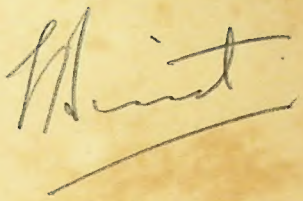


Brookside
Nelson.

MORE DIFFICULTIES
OF THE
EVOLUTION THEORY

" La théorie de l'évolution est impossible. Au fond, malgré les apparences, personne n'y croit plus et l'on dit, sans y attacher autrement d'importance, évolution pour signifier enchaînement, ou plus évolués au sens de plus perfectionnés, parceque c'est un langage conventionnel, admis et presque obligatoire dans la monde scientifique. L'évolution est une sorte de dogme auquel les prêtres ne croient plus pas mais qu'ils maintiennent pour leur peuple."

PAUL LEMOINE.

MORE DIFFICULTIES OF THE EVOLUTION THEORY

and a reply to "Evolution and Its Modern Critics"

by

DOUGLAS DEWAR, F.Z.S.

author of "Difficulties of the Evolution Theory," "Man: A Special Creation," "A Challenge to Evolutionists," "Indian Bird Life," etc.

LONDON
THYNNE & CO. LTD.
28—30 WHITEFRIARS ST., FLEET ST.
1938

Printed at The Riverside Works *of*
The Williams Press Limited, East Usk Road, Newport, Mon.

Nest of Indian Tailor-Bird in a Sterculia Leaf

NEST OF INDIAN TAILOR-BIRD IN A STERCULIA LEAF

See Page 97.

Frontispiece.

TO

DR. ALBERT FLEISCHMANN

Professor of Zoology and Comparative Anatomy in Erlangen

Who for over 37 years has openly opposed the theory of organic evolution

CONTENTS

Part I. Biological

CHAP.

I Creationism and Evolutionism.
II The Origin of Life.
III The Results obtained by Breeders of Animals.
IV Alleged Examples of the Origin of New Species in Nature.
V Some Transformations postulated by the Doctrine of Evolution.
VI The Recapitulation Hypothesis.
VII Parasitism and Metamorphosis.
VIII Nascent and Vestigial Organs.
IX Principles and Hypotheses.
X Reptiles and Birds.
XI Reptiles and Mammals.
XII Man.
XIII Difficulties presented by some Instincts.

Part II. Palaeontological

XIV The Dating of Geological Deposits.
XV The Absence of Fossils in Pre-Cambrian Rocks.
XVI The Palaeontological Record.
XVII The Origin of Families
- (a) The Equidae or Horse Family.
- (b) The Halicordae.
- (c) Some Molluscan Families.
- (d) The Anomiidae.
- (e) The Cypraeidae and their Allies.
- (f) The Limnaeidae and Valenciennesia.
- (g) The Rudists.
- (h) The Nassidae or Dog-Whelk Family.

XVIII The Nuculidae.
XIX "Evolution" within the Family.
- (a) Modifications of the Viviparids in the Levant during the Pliocene.
- (b) Gryphaea, Exogyra and other Oysters.
- (c) Inoceramus.
- (d) Volutocorbs and Volutospina.
- (e) Syringothyris.
- (f) Zaphrentis.
- (g) Micraster.

XX An Analysis of the Cases of Evolution cited by Dr. Davies.
XXI Conclusion.

LIST OF ILLUSTRATIONS

Frontispiece—Nest of Indian Tailor-Bird in a Sterculia Leaf.

Nineteen Specimens of the Shell of Purpura lapillus.
(Reproduced with the permission of Macmillan & Co. Ltd., from "The Cambridge Natural History.")
FACING PAGE 154

Some Varieties of Gold Fish.
(From sketches by Major W. R. J. Ellis.)
FACING PAGE 198

Charts: Pedigree of the Horse.
FACING PAGES 144, 156, 160

GEOLOGICAL TABLE

Cainozoic or Neozoic	Quarternary or Pleistocene or Holocene.	
	Tertiary	Pliocene Miocene Oligocene Eocene Palaeocene
Mesozoic	Secondary	Cretaceous Jurassic Triassic
Palaeozoic	Primary	Permian Carboniferous Devonian Silurian Ordovician Cambrian

Pre-Cambrian

PREFACE

In a book published in 1931 I endeavoured to show that the difficulties which beset the theory of organic evolution are so formidable as to render it desirable, if not to abandon the theory, at any rate to supplement it by a theory of special creation. Dr. A. Morley Davies, in his recently published volume entitled "Evolution and its modern Critics," has tried to meet most of the difficulties cited by me and has set forth facts deemed by him to be favourable to the theory. The present volume contains my reply to Dr. Davies and cites some further difficulties.

I submit that these three books taken together, present an outline of the arguments for and against the theory of which there is a pressing need, because almost every modern scientific book, written in English, dealing with evolution is a piece of special pleading for the theory. Nor is this all. The average biologist, accepting as he does evolution as a creed, fails, when writing, to distinguish between established fact and theory, and in consequence, sets forth theories as if they were proved truths. In my view such procedure is inexcusable in a treatise dealing with any science.

Whatever may be the demerits of my contributions to the discussion, they at least set forth a number of objections to the theory, and so, coupled with the books of Dr. Davies and of others supporting the theory, afford the student and the layman some sort of a basis

on which to found judgment as to the soundness or otherwise of the theory.

I am greatly indebted to Lt.-Col. L. Merson Davies, F.R.S.E. for reading the MS. and for valuable suggestions, to Major W. R. J. Ellis for his sketches of goldfish, and to Macmillan and Co., Ltd., for permission to reproduce, from the Cambridge Natural History, Dr. Cooke's illustration of the shells of Purpura.

PART I—BIOLOGICAL.

CHAPTER I.

Creationism and Evolutionism

From time immemorial there have been two schools of thought regarding the origin of the world of life—the creationist and the evolutionist. Until the majority of men of science became victims of Darwinian sophistry the creationist school had the greater number of adherents.

There is this difference between the two schools of thought; the Creationists do not hope to understand how God created the animals and plants, the Evolutionists, on the other hand, hope to discover not only the natural forces which they believe caused life to arise and assume its present diversities, but how these forces have accomplished this feat. So far this quest has been entirely unsuccessful.

Dr. Davies seeks to sustain the evolutionist philosophy.

He sets out by saying that I am silent as to the nature of the creative process and asks "Did the atoms of carbon, hydrogen, nitrogen and so forth of which the first created bird was built, exist previously to the bird itself? If so, in what combinations did they exist?" He concludes "Without some answer to these questions, the word 'creation' seems merely a fog, raised to conceal the difficulties involved."

Is this not a case of stone throwing by one who lives in a glass-house?

It is open to me to counter this attack by asserting that Dr. Davies is silent as to the nature of evolution and its causes and to ask "Is there any evidence that natural forces are now converting non-living into living-matter? If not, what justification is there for believing that these forces did so in the past? Knowing as we do, the chemical constituents of living matter, why cannot we produce this synthetically in the laboratory? Since one-celled organisms are able to thrive in most parts of the earth, why are not all organisms one-celled? Without some answer to these questions, the word 'evolution' seems simply a fog, raised to conceal the difficulties involved."

I, as a creationist, have the best of an argument on these lines, because it is open to me to say "As God's creative acts ended before I was born, it is unreasonable to ask me how they were effected, as unreasonable as to ask me how the wheel was invented (see page 22). Dr. Davies, may take refuge in the statement that the forces which caused life to originate and assume its diverse forms have ceased to operate. If he do this I retort "surely the chemist with his intelligence, learning, and appliances ought to be able to accomplish that which blind forces accomplished in the past."

I see no difficulty in an Omnipotent Deity creating animals and plants out of inanimate matter, or out of nothing, by mere fiat: "Let the waters bring forth abundantly the fowl that fly above the earth." But I find it impossible to believe that blind forces caused life to originate and assume its present diversity.

Dr. Davies writes (E. p. 249): "A belief in Evolution...in no way helps us to understand the mystery of conscious life; but for my part I cannot see that a re-

turn to belief in Creation will help us any better?" If this be so, then as regards this mystery there is nothing to choose between the theory of Creation and that of Evolution. But the fact remains that one of these is true and the other false. It is the business of the man of science to try to find out which is true. To rule out the possibility of creation and to act as though evolution were an established truth may be magnificent but it is not scientific.

CHAPTER II.

The Origin of Life.

In my "Difficulties of the Evolution Theory" I confined myself to the morphological and physiological difficulties which beset the theory that all the plants and animals are descended from one or a few common ancestors.

I made no mention of the great difficulty of the origin of the first living organism. All agree that at one time there were no living organisms on the earth. The Creationist asserts that the first living organisms were created miraculously, the evolutionist that they originated in the natural course of events, as the result of the interplay of forces or the fortuitous concourse of atoms and molecules. The latter view, unlike the former, can be tested experimentally. If in the past inorganic matter became transformed into a living organism, it ought to be quite easy to repeat the process in the laboratory. We know the chemical elements and compounds of which the bodies of animals and plants are composed. The bio-chemist has all these at his disposal. He can take any combination of them he chooses. Thanks to the apparatus at his disposal he can provide these compounds with any kind of environment he pleases, subject them to any sort of atmosphere, any degree of temperature and pressure, to any description of light or to complete darkness, to electric or galvanic treatment, to cosmic rays, to ultra-violet or

ultra-red rays, to X-rays and emanations from any radio-active mineral.

Nevertheless, so far all attempts to convert inanimate compounds into living matter have ended in failure. I doubt whether any scientific man to-day expects to be able to do so, because the extraordinary complexity of even the most minute living organism is realised. Gager truly remarks that the simplest cell, the unit of every organism, has a structure compared to which that of a modern printing press or a watch is simple and clumsy.

This, however, has not prevented some men from allowing their imagination to run riot. Thus Dr. C. C. Hurst writes ("The Mechanism of Creative Evolution" p. 292) : "That the protogene was the first manifestation of life upon this planet is highly probable, since no form of life is known to exist without genes. Precisely how the first genes arose, with the new power of growth and reproduction which is the all-important factor in life, we have no idea. Not only did they possess the power of growth but they also achieved the ability to keep it even when mutations so affected them that their whole reactions were changed. It seems most simple to suppose that they arose first of all by a fortunate combination of chains of carbon and other atoms present in that half-alive state mid-way between a self-producing chemical ferment and a living ultra-microscopic organism."

Dr. Hurst's protogene is an imaginary entity. He tells us that it is a primitive gene, but the gene, itself, is a theoretical entity invented to account for the phenomena of heredity; no one has seen a gene.

Speculation such as the above is laughable in a book

written for adults; but what adjective can we apply to the following passage in a school book by Dr. E. W. Shann and Mr. A. S. Gillespie ("School Certificate Biology," p. 12)? "Although, however, the atomic weights of the constituents of an organism are known, it has not been found possible to reconstruct the organism, much less set it in motion. But it does not follow that such a reconstruction will never be achieved. Indeed, although the claims of chemists to have created life artificially in the laboratory are generally discredited, living matter must have arisen from non-living matter at some period of the earth's history (unless to cover our ignorance we postulate its miraculous creation)...Matter does not live by virtue of the masses of chemical elements which are present in it at any given moment, but by the manner of linkage of those elements."*

Needless to state, speculations such as the above are not indulged in by all biologists. Sir Gowland Hopkins said (Pres. Address Brit. Assn. 1933): "Most biologists, I think, having agreed that life's advent was at once the most improbable and significant event in the history of the universe, are content for the present to leave the matter there."

Do those who believe that living organisms originated in the ordinary course of nature realise that the first organisms to have evolved would have nothing but water, atmospheric gasses and minerals upon which to feed?

There is no getting away from the fact that the

* As Messrs. Shann and Gillespie have not explained the difference in the "linkage" of the chemical elements in the body of an animal immediately before and after death, their assertion appears to be not scientific, but dogmatic.

failure to produce experimentally a living organism from lifeless matter is a most formidable difficulty of the evolution theory.

CHAPTER III.

The Results obtained by Breeders of Animals.

A telling piece of evidence against the evolution theory is the very limited changes breeders have been able to effect in the animals on which they have operated. Dr. Davies endeavours to show that this evidence is not so damning as I have made it out to be. He points out (E. p. 15) that "the range of form which artificial selection has been able to produce is far greater in some cases than in others—in dogs and pigeons, for instance, than in horses and cattle." "This" he asserts, "would seem to imply, on Mr. Dewar's theory, that the family limits are narrower in the latter cases than in the former."

This criticism is based on the misapprehension that I assert the family to be the unit of creation. For this misapprehension I am perhaps responsible, because in my "Difficulties of the Evolution Theory" I intentionally understated the strength of the experimental evidence against evolution when I asserted that breeders have not been able to produce a breed which can possibly be deemed to belong to a new family. I expressed myself thus to obviate cavil. Had I written "not been able to produce a new species" or even "not been able to produce a new genus," it would have been open to captious critics to charge me with understating the results obtained by breeders, and to support this charge by

citing some of the plant polyploids bred by geneticists and by quoting from page 17 of "The Origin of Species" : " Altogether at least a score of pigeons might be chosen, which, if shown to an ornithologist, and he were told they were wild birds, would certainly be ranked by him as well-defined species. Moreover, I do not believe that any ornithologist would in this case place the English carrier, the short-faced tumbler, the runt, the barb, pouter and fantail in the same genus."

As no one asserts that any of the breeds produced experimentally does not belong to the family of the wild form from which it is derived, I asserted that breeders have not produced a new family. This is not an admission that they have produced new genera or even species. If a species be defined as an interbreeding community, then no breeder has bred a new animal species. This does not apply strictly to plants on account of the phenomenon of polyploidy, as I have stated in "A Challenge to Evolutionists."

In my opinion Dr. Davies' selection of dogs and pigeons as being more plastic than horses and cattle is not very happy, because breeders of the former select freaks as the starting points of new breeds, while those of the latter select only useful variations. Nevertheless some species do appear to be more plastic than others—a fact that seems to tell against the concept of evolution, because, if all animals be descended from a common ancestor, those of the same degree of complexity should be equally plastic, except perhaps a few highly specialised forms.

I agree that the analogy between artificial and natural selection must not be pressed too far. From the fact that breeders, thanks to their intelligence and

knowledge and the apparatus at their command, can effect considerable changes in animals, it does not necessarily follow that the "blind forces" of nature can do likewise.

Dr. Davies' statement that the breeder deals with selected points only, while natural selection (or whatever effective agent we substitute for it) deals with the whole organism should, I submit, be restricted to commercial breeders. The geneticist deals with the organism as a whole, and can do what natural selection cannot, that is, forthwith isolate the individual he selects.

There is no getting away from the fact that, so far, breeding experiments afford evidence most unfavourable to the theory of evolution. Contrary to the expectations of evolutionists they demonstrate the great stability of species. Truly does Prof. Caullery write (Pp. 240) : " The greatest difficulty at the present time is to reconcile this stability with the mutability that the very notion of Evolution supposes."

Let me here clearly state my views regarding the units of creation. I do not assert that every species, or every genus, or, even, every family has been specially created. I do not know what the units of creation are; it may be that they do not exactly coincide with any of the present systematic categories. It is the duty of the biologist to try to discover what these units are. In my opinion the data at present are not nearly adequate to enable anyone to make even a tentative pronouncement on this matter. All that can be said safely is that, so far, breeding experiments seem to indicate that the units of creation are small, or, in other words, that the number of these units is great.

CHAPTER IV.

Alleged Examples of the Origin of New Species In Nature.

There is no *proof* of the origin of a new species in nature. This assertion may well startle those who have read such statements as the following by Professor E. R. Goodrich, F.R.S. (B. Vol. 8, p. 917) : "It is now universally held by competent biologists that all organisms, living or extinct, have arisen from remote common ancestors by a process of gradual change or evolution, and further, that living matter or 'life' itself, in all probability arose from non-living matter in the first stages of this evolutionary process."

Nevertheless, I believe what I have stated to be correct. I have been unable to find any proof of the origin of a new species in nature. The only attempts to demonstrate this that I have come across break down on examination.

Professor E. A. MacBride asserts ("Evolution" [1932] ; p. 33) ; "Evidence has come in which completely substantiates Darwin's theory that sterility is a by-product of increasing difference of constitution...as this point is a fundamental one, we shall give two instances of it. There is in Europe a large frog, the so-called edible frog (Rana esculenta), with a greenish skin diversified by black patches. Of this frog at least three races are known to exist—viz. a western in France, Spain and Portugal, a central in Germany, Belgium and

Holland, and an eastern in the Balkan Peninsula and Syria. Each of these races breeds with its neighbour on the border of its territory, and there intermediate types are common. But the eastern race has spread along the north shore of Africa and has reached the southern part of Spain where it meets the western race, and in this territory the two do not interbreed.

It is obvious that, if the central race were wiped out, the eastern and western races would rightly be regarded as two distinct species."

This statement is inaccurate. The true facts are; there are three European races of this frog : var. typica, which seems to be confined to Central Europe and Italy, var. ridibunda found in Spain and all other parts of Europe except England, Northern France, Denmark, the Rhine Countries and Italy, also in Asia as far east as Afghanistan and in North Africa from Egypt to Morocco, and var. lessonae, which is sparsely scattered in Europe, but of which I can find no record in Spain. These races where they overlap, do not ordinarily interbreed, for the very good reason that they have different spawning times; but Pfluger succeeded in crossing typica and ridibunda, and progeny resulted from the cross. I pointed this out to Professor MacBride and he, on looking into the matter, admitted that he was mistaken and said he would correct the mistake if a further edition of his book were called for.

This being so, I should not have cited this case, but for the fact that, had I failed to do so, my assertion that there is no proof of the origin of a new species in nature might have been challenged. The fact that the best that Professor MacBride can do when attempting to prove the origin of a new species in nature, is to cite

this frog, tends to show that it is not possible to obtain proof of such origin.

Let us now notice Professor MacBride's second attempt to prove his thesis. "There is" he writes (Loc. cit., p.34), "a fairly common moth known as the gipsy moth (Lymantria dispar). This moth is spread all over the world from the New England States through Europe to Japan. All the local races will breed with one another, but when two distinct races are crossed the sex of the offspring is influenced by one race more than the other. This preponderant race is called the strong one. If the father comes from this race the hybrids will consist of males and of females with a good many marks of the male. Such masculinized females are known as 'inter sexes.'

In many cases these intersexes are so much altered that they are unable to produce good eggs, and so the cross-breeds are unable to propagate themselves and the two parent races are as mutually sterile as two species, while the less distant races are still mutually fertile. It follows that there is every gradation between a race and a species, and as races owe their differences to the different effects of their surroundings, so the distinctness of species may be assumed to be due to the long-continued and deeply ingrained influence of different surroundings."

If Professor MacBride suggests, as he seems to do, that some of the races of the gipsy moth are as mutually sterile as two species, I have no hesitation in saying that this is not the case. It is true that when a strong and a weak race cross the offspring of one sex only are affected and may become intersexes, but these are not all sterile.

The peculiarity of producing intersexes is exhibited by other kinds of moth. It is due in part to the fact (see p. 27) that the embryo exhibits the primordia of the organs of both sexes, but normally only one set matures.

G. Goldschmidt, who has done much work in connection with the gipsy moth, shows that an intersex is an individual which has developed as a male (or a female) up to a certain point and then continued its development as a female (or a male). In some races the genes or factors which determine sex are stronger than in others, so that, if members of a strong race mate with those of a weak, the balance of sexual development may be upset. The Japanese race is strong and the European weak. If a male Japanese moth be crossed with a female European moth the male offspring are all normal, but the females have certain male characteristics, owing to the fact that the strong male sex-determining factors work more quickly than the female, and overtake them during the development of the female. In the reverse cross between a Japanese female and a European male all the offspring are normal, but on breeding from these a certain proportion of males are produced having female characters. This is in accordance with Mendel's law. "Further work" writes Dr. C. C. Hurst ("Mechanism of Creative Evolution" p. 242), "showed that among the many races of the moth all degrees of sex 'strength' or 'weakness' relative to each other occur, each race breeding normally itself but producing different results on being out-crossed to other races. Thus, if a race graded as producing 'very strong' males is mated to females of a race with 'weak' femaleness, all the offspring are male in their

type or organisation. If a 'strong male' is mated to a 'weak female,' half the offspring are normal males, the other half intersexes, and so on. In this way by suitable inter-racial breeding it is possible to get every type of intersex, from one almost male to one almost female, in addition to normal males and females."

Dr. Hurst suggests that, as the chromosome number (62) is high in the gipsy moth, "it seems not unlikely that this curious quantitative effect of the genes determining sexual characters may be due to a condition of masked polyploidy, which would involve the presence of more than one set of sex chromosomes...some races having more male elements, while others have more female."

Thus the gipsy moth does not afford an instance of a new species arising in nature. All the races are perfectly fertile *inter se* and a large proportion of the offspring of such crosses are perfectly fertile when crossed with the parental or any other race.

If a species be defined as an inter-breeding community we have yet to find *proof* that a new species of animal has arisen in nature.

One of the most remarkable phenomena of the living world is the purity of the species. Hybrids are very rare in nature. The only wild-bred cross between truly distinct species that is at all common is the Rakkelhane or Blackgame-Capercaillie hybrid. It is true that the pheasant in England sometimes crosses with the common fowl, but neither of these is in its natural habitat.

On every hillside and in every meadow scores of different species of plants grow side by side, each retaining its purity, despite the fact that the pistils of many

flowers must receive pollen from other species. Contrast this with the elaborate precautions the horticulturalist has to take to secure the purity of the varieties he has raised.

Professor J. B. S. Haldane goes so far as to say that the "barrier of inter-specific sterility is the most serious argument against Darwin's Organic Evolution" ("Possible Worlds" p. 38). This and other facts have led a number of biologists to abandon the Darwinian conception of evolution and to adopt the idea of lineages which pursue a parallel course through the ages. Thus Berg writes (N. p. 358): "To support the view that animals descended from four or five progenitors is now impossible: the number of primal ancestors must be computed in thousands or tens of thousands." According to Berg, Belogolovy speaks of "millions of initial points." A theory such as this meets the difficulty of inter-specific sterility, but is beset with many difficulties, some of which will be discussed later. (See p. 95.)

CHAPTER V.

Some Transformations Postulated by the Doctrine of Evolution.

As examples of transformations postulated by the doctrine of evolution which appear to be impossible on physiological or anatomical grounds I cited (D. p. 67) the gradual conversion of (1) an Amphibian into a Reptile, (2) a Reptile into a Bird, (3) a Reptile into a Mammal, (4) a land mammal into a Whale, (5) a lower animal into a Human Being.

Dr. Davies has dealt specifically with nos. 1, 3, and 4 of the above cases. My comments on his remarks respecting case no. 3 are contained in Chap XI. Before discussing his remarks on nos. 1 and 4 I will reply to Dr. Davies' criticism of Vialleton's assertion that it is inconceivable that a flower has been evolved gradually.

Dr. Davies points out that the following types of plants exist: (1) those (Ferns) the leaves of which bear sporangia of one kind only, (2) those (Selaginella) that bear sporangia of two kinds, male and female, and (3) those (Cycads) in which the sporangia are borne on cones, the male on one plant and the female on another.

This is no evidence of evolution, i.e. that the first type of reproduction changed into the second and the second into the third. The fact that fish, amphibia, reptiles and mammals are now living and that the fish appear first in the rocks known to us, then the amphibia,

then the reptiles, and finally the mammals is not proof that each successive member of that series is derived from its predecessor. Indeed as Bergson remarks the fact that it is possible to constitute an evolutionary series out of the evolved militates against the theory of evolution.

As the notion that, in the past, some kind of fern changed its mode of reproduction is contrary to the observed behaviour of ferns, it is unreasonable to ask us to believe that this change actually took place, in default of conclusive fossil proof that it happened. It cannot be too strongly emphasised that succession does not necessarily imply descent.

As regards the supposed transformation of an amphibian into a reptile, I mentioned some of the changes involved and said that it is apparently impossible that all these took place gradually and in such a way as to fit in with one another, and I then asked; "How did the embryos contrive to survive during this long period of transition?" Dr. Davies makes no attempt to answer this question. He writes (E. p. 48) : "Here we have a pure assumption of the impossibility of there being a gradual series of stages at each of which the egg and the developing embryo were fully 'viable.'"

This is an attempt on the part of Dr. Davies to fit the boot on the wrong leg. Let us examine his attitude. A nursery rhyme tells us that the dish ran away with the spoon and the cow jumped over the moon. Now, although a book I once possessed contained pictures exhibiting these performances, I think it impossible that they ever happened. Dr. Davies, if he be logical, will tell me I am wrong, that I make a pure assumption of the impossibility of these things. He

asserts positively that in the past an amphibian became gradually transformed into a reptile and calls upon me to prove that this did not happen. I submit that the burden of proof lies upon him who asserts. In the case of this supposed gradual transformation the burden of proof lies on Dr. Davies. Until he can furnish this, he makes a pure assumption. Unfortunately this assumption is of such a nature that it can be neither proved nor disproved. Whether or not it is probably correct depends entirely on the amount of evidence that can be adduced in support of it. *Prima facie* it is in the highest degree improbable that such a transformation has been effected gradually. If, however, it could be shown that geneticists have effected the transformation and, at the same time, a series of intermediate fossils were produced, there would then be a strong presumption that the assumed transformation did actually occur.

I feel constrained to comment on the manner in which evolutionists slur over the difficulties in connection with the transformations which they suppose animals to have undergone.

In my "Difficulties of the Evolution Theory" I dealt mainly with those of an anatomical or mechanical nature. Of the physiological difficulties I merely cited the acquistion of the power of keeping the blood at a constant temperature and the changes in the nature of the products of excretion.

As regards the first of these difficulties Dr. Davies contents himself with pointing out that the body temperature of monotremes is lower than that of marsupials and placentals, and that, at high temperatures, the control of the former is less effective than that of the latter. This does not explain how or why the control in any

form was acquired. Nor is any light thrown on the matter by his question; Is there anything unreasonable in the supposition that, if we could restore some of the Triassic mammals to life, their control would be less than that exercised by a monotreme?

Dr. Davies rightly considers my statement "Amphibia and Mammals excrete urea and Reptiles and Birds uric acid" to be too sweeping a generalisation, and the facts to be not so simple as this implies. This fact, however, does not resolve the difficulties: it enhances them. In this connection a contributor to "Nature" who signs himself "J.S.G." wonders if I understand the basal phenomena of nature. I certainly do not. I do not understand why, when two molecules of Hydrogen gas unite chemically with one molecule of Oxygen gas, the resulting compound has the qualities of water, nor do I understand what gives to the basic matter of life the properties exhibited by it. Some people evidently think they do. E.g. Messrs. Shann and Gillespie, (vide p. 6.)

For my part, far from pretending to understand it, I am greatly impressed by the complexity of the simplest living organism and of the chemical processes that go on within it.

Many evolutionists seem to labour under the delusion that the discovery of the means whereby certain physiological processes are effected is tantamount to discovering how these functions originated.

For example, as Dr. Baldwin points out ("An Introduction to Comparative Biochemistry" p. 24), fresh-water invertebrates are prevented from bursting owing to an excessive intake of water by being provided with a non-permeable cuticle, save over a small permeable

area, and an excretory organ capable of secreting hypotonic urine, and therefore of doing osmotic work. These devices do not occur in marine invertebrates. This knowledge does not tell us how any marine animal acquired them. But the evolutionist goes farther than supposing that this happened, he believes that some freshwater animals in the past took to a terrestrial existence. Dr. Baldwin admits that in order to effect this change of existence "many difficulties" had to be overcome. Among these were the "ever-present danger of water shortage", the modification of the respiratory organ so as to allow of air-breathing, the development of a new arrangement for ridding the organism of the ammonia it secretes. These changes present no difficulty to Dr. Baldwin, who writes (p. 35) : "Adaption to terrestrial life called forth important changes in the mechanisms of nitrogenous metabolism."

Those who have seen the play "Alf's Button" will remember that every desire expressed by the worthy Alf was forthwith gratified. The evolutionist seems to deem the aquatic animals that took to living on land even more fortunate than Alf, for when he wanted anything he had to know what he wanted and to ask for it; the transforming aquatic animal was not under this necessity; the fact that it needed a thing sufficed to cause this to materialise! My view is that, if ever an aquatic animal became transformed into a terrestrial one, the transformation must have been effected miraculously.

My criticism of the supposed gradual transformation of a land mammal into a whale is (D. p. 61) that this appears to be physically impossible, because the tail could not act as a propeller by vertical motion until the pelvis had become so small as to render locomotion on

land impossible. I asked: how could the creatures at the half-way stage of transition have held their own in the struggle for existence? I also quoted (D. p. 77) Vialleton's assertion that it is an illusion to look for such intermediaries.

Dr. Davies meets my criticism thus (E. p. 92): "Admittedly we have here a 'difficulty of the evolution theory.' While links are missing it is difficult to picture their exact mode of life. Precisely the same difficulty occurs when we try to picture how the wheel was evolved from the roller: the exact nature of the intermediate stages have so far baffled all attempts at reconstruction. The easiest way out of the difficulty would be to give up the attempt and say that the wheel was not a human invention but a supernatural revelation; yet I know of no one who has adopted that view. Everyone believes that the wheel was developed out of the roller, though no one can confidently say how."

If it be true that we cannot imagine how man invented the wheel, it would mean either that the wheel was "a supernatural revelation," or that ancient man was more intelligent than his present day descendents. The latter alternative does not fit in very well with the theory of evolution!

Moreover, the conversion of a roller into a wheel is a mere bagatelle in comparison with that of a land animal into a whale. The former during the period of transformation had not to find its food or to hold its own in competition with other rollers or wheels. I cannot conceive how a land animal became gradually converted into a whale or sea-cow, but I have no difficulty in imagining the various stages of the conversion of a log into a spoked wheel, or in making rough sketches of these. I

do not say that my sketches would represent the stages actually passed through. But I do not challenge evolutionists to make sketches of actual ancestors of the whales. I ask for drawings of skeletons of *possible* intermediaries. So far no one has taken up my challenge. Mr. Arnold Lunn is more fortunate. It is recorded ("Science and the Supernatural" by Lunn and Haldane, p. 320) that in response to this challenge Professor J. B. S. Haldane pleaded that he is not a good sketcher, but that his drawings would be rather like caricatures of dugongs and seals. Now the dugong, being as well adapted to swimming with the tail as the whale, can scarcely be called an intermediary between the latter and a land mammal. The seal, although adapted to existence both on land and in water, is not anatomically intermediate between a whale and a land animal. Let us notice what would be involved in the conversion of a land quadruped into, first a seal-like creature and then into a whale. The land animal would, while on land, have to cease using its hind legs for locomotion and to keep them permanently stretched out backwards on either side of the tail and to drag itself about by using its fore-legs. During its excursions in the water, it must have retained the hind legs in their rigid position and swum by moving them and the tail from side to side. As a result of this act of self-denial we must assume that the hind legs eventually became pinned to the tail by the growth of membrane. Thus the hind part of the body would have become like that of a seal. Having reached this stage, the creature, in anticipation of a time when it will give birth to its young under water, gradually develops apparatus by means of which the milk is forced into the mouth of the

young one, and, meanwhile a cap has to be formed round the nipple into which the snout of the young one fits tightly, the epiglottis and laryngeal cartilage become prolonged upwards to form a cone-shaped tube, and the soft palate becomes prolonged downwards so as tightly to embrace this tube, in order that the adult will be able to breathe while taking water into the mouth and the young while taking in milk. These changes must be effected completely before the calf can be born under water. Be it noted that there is no stage intermediate between being born and suckled under water and being born and suckled in the air. At the same time various other anatomical changes have to take place, the most important of which is the complete transformation of the tail region. The hind part of the body must have begun to twist on the fore part, and this twisting must have continued until the sideways movement of the tail developed into an up-and-down movement. While this twisting went on the hind limbs and pelvis must have diminished in size, until the latter ceased to exist as external limbs in all, and completely disappeared in most, whales.

Besides this effort of the imagination on the part of Professor Haldane the story of Cinderella becomes positively banal. Truly does Mr. A. Beebe write ("The Bird" p. 97): "The idea of the miraculous change, which is supposed to be an exclusive prerogative of fairy tales, is a common phenomenon of evolution." Indeed Professor A. Fleischmann attributes the ready acceptance of the theory of evolution to the fact that the love of fairy tales is deeply implanted in the heart of man!

I feel sorry for Professor J. B. S. Haldane because

he was driven by Mr. Arnold Lunn to face this difficulty which evolutionists with one accord slur over. A delightful example of this occurs in the late Sir J. Arthur Thomson's "Biology for Everyman." He tells us that whales are descended from land mammals that took to the water. He writes "We may begin with an animal like the stoat that occasionally jumps into the water and swims well. The next step may be illustrated by the otter, that is thoroughly at home in the river and may swim for miles out to sea, yet remains equally at home on land. On the next level may be placed the almost exterminated sea-otter (Enhydris) of the North Pacific, whose hind feet are suited only for swimming. Then we reach the progressive series represented by sea-lion, walrus, and seals—the last being almost as thoroughly aquatic as the whales, except they bring forth their young on the shore and nurse them there."

The above passage, while in no way resolving the difficulty of the transformation of a land into an aquatic mammal, contains the fallacious assumption that evidence of the transformation is afforded by the fact that some existing mammals are more aquatic than others.

CHAPTER VI.

The Recapitulation Hypothesis.

Dr. Davies' remarks on the recapitulation theory are most interesting and I hope that they will be widely read. He candidly admits that (E. p. 138) : "in any strict sense the recapitulation of ancestral history is a sheer impossibility." He also admits that recapitulation may not only be abbreviated, but unequally abbreviated, stages may be skipped or short-circuited and even "something like a reversal of ancestral stages may sometimes occur." Nevertheless he does not discard the theory; his view is "The structural changes through which an animal passes in its ontogeny, if they are not accounted for exclusively by the immediate necessities of life, are a valuable indication of the ancestral history."

Even in this diluted form the theory does not hold water. The alimentary canal in the embryo for some time is devoid of mouth and anus. This cannot represent an ancestral state. What is the immediate necessity of life that requires the primitive gut to be a blind tube?

If there be a law of embryonic development, I submit that it runs thus : The structural stages through which an animal passes in its ontogeny follow the shortest and most direct route possible to the adult state, compatible with the immediate necessities of life.

In order to continue the species the problem to be

solved, in the case of animals in which the male and female organs are in different individuals, is the production of offspring inheriting the characteristics of both parents, with a minimum demand on their tissues, and to provide every cell of the growing embryo, throughout the period of development, with a constant supply of nourishment and oxygen and a means of ridding itself of the waste products of metabolism.

In the case of sessile animals there is the further problem of securing for them the means of dispersal. The necessity of solving these problems affords full and sufficient explanation of most of the phenomena of embryonic development.

There are several conceivable methods whereby the fertilised cell could acquire all the organs of the complex adult. The particular method adopted is to endow the original cell with the power of developing, at a very early period, the tissue-producing cells or primordia (or, as the Germans say, anlagen, or the French ébauches) for each of the major organs or structures that occur in any member of the class of animal to which the embryo belongs, even the primordia of structures which that particular individual will not need in the adult state. In the case of the latter the development of the primordia capable of producing them is early checked; thus the embryo of every higher animal exhibits the primordia of both the male and the female generative organs, but in normal circumstances only those of one sex attain maturity. The absurdity of the notion that the embryo of a higher animal recapitulates a hermaphrodite ancestral stage was exposed by Prof. Crew's famous hen, who, after the termination of her egg-laying period, assumed male characters and became

the father of a brood!

We have noticed that the embryo must have a continuous supply of nourishment during the whole of its period of development. In the case of mammals, where the mother affords this nourishment, and of birds, where the egg contains an adequate supply of food yolk, the embryo proceeds direct to the adult form.

When, as in the case of many invertebrates, the egg does not contain food sufficient for the needs of the embryo during the whole of its development period, the embryo hatches out in an incompletely-developed state as a free-swimming larva able to find the food necessary to complete its development. Let us consider, in the light of these observations the points raised by Dr. Davies.

He asserts (E. p. 145) that the blood circulation system of the vertebrate embryo exhibits "a fish plan, elaborately adjusted to meet the needs of an air-breathing animal." To this I demur. No known fish has a circulatory system like that of the vertebrate embryo at the time when the blood runs through the series of aortic arches, each of which is a single tube conveying blood from the ventral to the dorsal part of the body. In fishes the blood system in the pharyngeal region is a complicated arrangement, consisting of a series of arches, each of which contains an afferent and an afferent vessel connected by a network of capillaries that enter the gills where the blood they contain is aerated. If the early aortic-arch system of the vertebrate embryo can be said to be on the plan of any other animal, it is the annelid plan. The earthworm has in each of the 7th, 8th, 9th, 10th and 11th segments an "arch" or blood vessel, known as a heart, conveying blood from the

dorsal "aorta" to the ventral (sub-intestinal) blood vessel. It is true that in the earthworm these vessels themselves are pumping organs, in consequence no special heart exists, also that the blood flows in the opposite direction from that in the vertebrate embryo, but in the earthworm the central nervous system is situated ventrally instead of dorsally, so that in each case the aortic "arches" convey blood to the main nervous system. If the dorsal and ventral parts of the earthworm were reversed, the early blood system in the anterior part of the vertebrate embryo would be a replica of that of the annelid. Indeed largely on this account, some have deemed vertebrates to be derived from an annelid ancestor. Thus, had Dr. Davies stated that the circulation of the vertebrate embryo is on the annelid plan, elaborately adjusted to meet the needs of, on the one hand, a gill-breathing, and on the other a lung-breathing animal, I should not have been able to retort that the blood system of the vertebrate embryo is not at all like that of an annelid, but I could have cited grave objections to the theory that vertebrates are descended from annelids.

I submit that the circulatory system in the vertebrate embryo is peculiar to vertebrates and a most effective method of providing the embryo with a blood supply at a very early period by means of an apparatus capable, while it is serving its purpose in the embryo, of developing into the type of circulation required by the adult, be it fish, amphibian, reptile, bird or mammal.

I asserted (D. p. 48) that, as the 3rd and 4th embryonic visceral arches give rise to no structures in any existing amphibian, reptile or bird, the evolutionist has to suppose that they exist only because the embryo is ob-

liged to recapitulate the early fish stage. This being so they ought to have undergone atrophy long ago. I added "the recapitulationist has to suppose that they did not undergo atrophy, but after millions of years suddenly acquired the power of developing into the epiglottis and contributing to the formation of the thyroid cartilage (Adam's apple) in mammals." To this Dr. Davies retorts (E. p. 151): "The fallacy here is, as in other arguments of Mr. Dewar's, that he assumes that because in *existing* adult amphibians and reptiles these two visceral arches have disappeared from want of function, that they had already disappeared or were functionless in those primitive amphibia and reptiles through which mammals are derived."

Dr. Davies suggests that these arches gave rise to some structures in the primitive amphibia and reptiles from which he thinks mammals are derived. He does not even hint as to the nature or uses of these structures imagined by him. His suggestion, which to me seems far-fetched, cannot be proved or disproved. It must mean, either that the amphibia and reptiles in the direct line of mammalian ancestry possessed unknown structures peculiar to them, which later became converted into the epiglottis and thyroid cartilage in mammals, or that all the primitive amphibia and reptiles possessed these structures which they subsequently lost without coming to any harm, and either that this loss occurred before birds evolved, or that birds originally possessed them and have lost them.

It is worthy of remark that no amphibian, reptile or bird embryo recapitulates the late stages in the development of these hypothetical structures: thus it would seem that Dr. Davies believes that useless visceral

arches and blood vessels appear in the embryo because it obeys the law of recapitulation as regards the earlier but not the later stages of development. My view is that these two arches, with their blood vessels, develop because they are essential to the provision of an adequate supply of blood to the head region of the embryo at an early stage.

I ask those who believe that all but one of these arches are useless to the embryo and only develop because the embryo has to recapitulate a fish stage; why does not the reptile, bird or mammal embryo recapitulate the formation of the gills, their afferent and efferent blood vessels and capillary system?

Dr. Davies writes (E. p. 146): "If therefore we accept Mr. Dewar's interpretation that creative activity is under compulsion to conform to a certain 'Vertebrate plan' we must infer that the plan was originally chosen with a view to the creation of water-breathing fishes, and that the air-breathing vertebrates were an afterthought, the original fish plan in them being patched up in various ingenious ways to suit cold-blooded and warm-blooded air-breathers. If the evidence were in the contrary direction—if the fish circulation showed features which could only be explained as modifications of a plan primarily designed for air breathers, it would provide an argument for creation. As it is, the opposite is the case."

As regards the vertebrate plan, I suggest that this was deliberately chosen, not that the creative activity was under compulsion to conform to any plan. I have already shown that the plan is of such a nature as to embrace the formation of both gilled and lunged vertebrates.

If the fish circulation showed features which could only be explained as modifications of a plan primarily designed for air-breathers, in my view, far from affording an argument for creation, this would have shown the creation of fishes to be an afterthought! But this is beside the point; I assert that there is nothing fishy or fish-like in the early phases of the development of the embryo of an air-breathing vertebrate. There is a phase common to gill- and to lung-breathers, after which the development of each type follows a different course.

Let us now examine the reasons given by Dr. Davies for believing that an original fish plan has been patched up in order to suit air-breathers.

The first of these reasons is that the heart of a bird or a mammal is a double organ, so much so that physiologists find it convenient to speak of a right and left heart as though they were separate organs. Dr. Davies asserts that there is no physiological reason why they should not have been created separate hearts, like the systemic and branchial hearts of the cuttle fish.

I submit that the formation of two hearts at the inception of the embryonic circulatory system would have been a clumsy arrangement, necessitating each heart being perforated laterally and connected by a passage in order that the embryo should have an effective systemic blood circulation during the period in which the pulmonary circulation is small. Later the perforations of the hearts would have had to be closed and the connecting passage would become a superfluity. Recapitulationists seem to lose sight of the fact that the needs of the developing embryo are quite as important as those of the adult.

The reason why Dr. Davies deems the original embry-

onic plan to have been patched up is the fact that in air-breathing adult vertebrates there is no need for more than one aortic arch (half a pair), nevertheless there are several pairs in the embryo. Here again Dr. Davies seems to have overlooked the needs of the developing embryo. It is imperative that this be given a blood circulation at a very early stage; it cannot wait for this while one big aorta is being developed. This is why it is provided with several pairs of arches.

Dr. Davies deems the existence of a tail in the human embryo to be incompatible with the theory of special creation. "Man" he writes (E. p. 223) "has only a slight vestige of a tail, but in the foetal stage this tail is not only proportionately much longer but provided with the muscles found in animals with movable tails. The creationist must either show that this tail serves some useful temporary purpose; or he must fall back on some fanciful explanation as Vialleton used for the bird's wing."

I take exception to the words "man has only a slight vestige of a tail," because they set forth an unproved theory as if it were a demonstrated fact. I submit that the correct statement should be, "Man has what many biologists believe to be a vestige of an ancestral tail," or, better still, "Man has no tail, but many believe his os coccyx to be the relic of a well-developed ancestral tail." It may be noted that the os coccyx is situated lower and is longer in man than in the anthropoid apes, being composed of four vertebrae in the former and three in the latter. Thus, if man descended from an anthropoid ape, his os coccyx has increased in size. As I have pointed out (M. p. 37), these differences correspond to profound differences between the organisation of man

and that of the anthropoid apes. The creationist view is that man has no tail and that the primordium that gives rise to the tail in tailed vertebrates gives rise to the os coccyx in man.

But I agree that the creationist has to explain why the human embryo has at one stage a fairly long tail, if a tail be defined as part of the animal behind the hip-girdle. This I will now do.

The animal kingdom is divided into more than a dozen great groups which have been given the question-begging name of phylum. Each of these phyla is constructed on a fundamentally different plan.

In the majority of the phyla, after the original cell has become subdivided into more than a thousand cells, these arrange themselves into three basic layers called germinal layers, known as the ectoderm, mesoderm and endoderm, which enfold each other. Every vertebrate embryo is, so to speak, built up round the notochord—the precursor of the backbone. The neural axis is the first product of the ectoderm and the primitive proto-vertebrae that of the axial mesoderm. These proto-vertebrae are formed in almost the whole length of the body; the embryo at this stage exhibits a chain of segments or metameric primordia (anlage), the function of most of which is to produce vertebrae, muscles and nerve fibres. These are indispensable to the embryo, but are purely embryonic structures. Although in outward appearance all are alike, each develops on peculiar lines. The members of this series of primordia situated behind the spot at which the hind limbs bud out from the embryo give rise to the tail of the adult, be it long or short. According to Sir Arthur Keith the human embryo displays from eight to eleven of these incipient

tail vertebrae. Kunitoma, who has made a special study of the development of the human tail, records that the longest tail he has seen measured 1.2 mm. in an embryo 7.5 mm. in length. This tail, at early stage, is composed of a longer basal part containing primitive vertebrae, the chorda dorsalis and the middle sacral artery and vein, and a shorter distal part in which the tissue is not differentiated. The tail begins to diminish in size when the embryo is from 8 to 9 mm. in length, and by the time it has attained a length of 25mm. the whole of the vertebrated part of the tail has become withdrawn into the body, leaving exposed only the distal part as a pimple placed a little dorsal to the spinal column. This portion is eventually absorbed. Of the vertebrae in the other part of the tail four usually continue to develop and fuse to form the os coccyx, which soon becomes bent forward and serves for the attachment of certain muscles. The fact that the tail is thus curved enables man to sit without suffering inconvenience and probably affords some of the additional support required by the viscera on account of man's upright posture. It is important to notice that in its earliest state the tail of man is as long as that of the embryo of a long-tailed animal. If man be descended from a long-tailed ancestor, which had gradually lost the greater part of its tail, it is reasonable to suppose that this organ would have gradually become shorter at its inception, but, as we have seen, this does not happen.

If anyone assert that the human embryo recapitulates the various stages of its tailed ancestors, I should say to him "You assert that the whale is descended from ancestors having well-developed hind legs, why

does the whale embryo not exhibit, at an early stage, well-developed legs and which later shrivel up?

In connection with the human embryonic tail, it is important to bear in mind that at an early stage, i.e., before the second month in man, the human (and indeed every vertebrate embryo) exhibits a length of intestine behind the vent or anus. He who asserts that the human embryonic tail is a relic of a tailed ancestor, must, if he be logical, assert that the post-anal gut is a relic of an ancestor that went through life having such a strange organ. Writers who dilate upon the human embryonic tail are usually silent regarding the post-anal gut. Is it fair to students to withold this fact from them? Clearly both embryonic tail and post-anal gut appear because of the way in which embryos develop.

It may be asked, why does the tail develop in this way: why does it not develop as legs do, by being budded from the body? The reply that is that the tail whether it extend beyond the body as in most animals, or is enclosed within the body, as in man and the anthropoid apes, contains several vertebrae, and obviously it is more simple to produce these at the same time and in the same way as the other vertebrae than to develop them in an entirely different manner.

We must bear in mind that there is no such thing as waste in nature; it is not wasteful to develop structures that are subsequently absorbed or for any animal to lay millions of eggs, few of which come to maturity. In each case the material employed is subsequently utlised, again and again.

Dr. Davies seems to be under the impression that the innervation of the retina of the vertebrate eye is difficult for Creationists to account for. I do not agree.

The nerves of the vertebrate retina are situated between the source of the light falling on the retina and the photosensitive parts of the latter—the rods and cones. According to Dr. Davies this arrangement affords evidence of the evolutionary origin of the eye, it exists only because the eye in its embryonic development recapitulates what he believes to be the various forms of a series of ancestors. I do not believe that any animal existed having an eye in the condition depicted in fig. no. 29, C. and D. of Dr. Davies' book. I assert that the vertebrate eye develops in such a way that it could not function as an eye until its development is complete, or very nearly so. It is one of the organs which cannot be deemed to recapitulate the ancestral stages postulated by evolutionists.

As to the relative positions of the nerves and the rods and cones, I know no mechanical or physiological reason why both nerves and rods and cones should not all have developed in the lower layer of the optic cup, the upper layer becoming absorbed, or, as happens in the case of Sphenodon mentioned by Dr. Davies (E. p. 229), developing into the lens. The fact that this does not happen, that the method of development followed in the pineal eye of Sphenodon is not adopted in this case, indicates that there is some advantage in the present positions of the sensitive parts of the retina. As to the alleged inconvenience of the "blind spot" in the vertebrate eye, I shall be beholden if anyone can furnish me with a single instance of an animal being captured, or missing food owing to its blind spot.

Dr. Davies is of opinion that one nerve could supply the whole eye, and, as this organ is served by three nerves and six muscles to which three embryonic metameres or

segments contribute, he deems this evidence for evolution. I do not know what his reasons are for asserting that two of the three nerves are superfluous. As to the eye nerves and muscles being derived from three of the segments, since the optic vesicle extends into three of these segments the function of which is to produce nerves and muscles, it is but natural that each of them should furnish the nerves and muscles to the portion of the eye within its ambit. Had each segment supplied one nerve and two muscles Dr. Davies might have asserted that each was mechanically providing its quota, but this is not so: the foremost of the three contributes four muscles and the others one apiece. Here we have an example of the beautiful way in which the various parts of the body work in co-operation, as if directed by some outside agency.

CHAPTER VII.

Parasitism and Metamorphosis

Two difficulties of the evolution theory not mentioned in my former book are the supposed evolutionary origin of the parasitic habit, particularly in the case of endoparasites which spend part of their existence in one kind of host and part in another kind, and the metamorphosis undergone by many animals after they have emerged from the egg. In the latter case the evolutionist has to explain why any creature that hatches out as a free-swimming larva able to find food and fend for itself becomes transformed later in life into quite a different type of animal.

Sacculina in the adult state is little more than an egg-producing bag provided with filamentous excrescences by means of which it obtains nourishment from the crab on which it is parasitic. It leaves the egg as a free-swimming larva. Dr. Davies writes (E. p. 144): "Granted that Sacculina must have a free-swimming larva, why should it not have a ciliated larva like a Trematode, or like so many Echinoderms, Worms and Molluscs? Why does it have two successive larval stages, thereby increasing the risk of death, before reaching the host? Why should these larvae be of Arthropod type, moving by muscular appendages and entirely devoid of cilia? Why should the first larval form be one common to most of the lower Crustacea, and

the second be characteristic of the ordinary non-parastic Cirripedes? Evolution gives a meaning to all these peculiarities: Creation can only suggest a storage place with an inadequate number of pigeon-holes, so that a parasite has to get shoved in along with some group—no matter which—to which it has no resemblance."

The reason why the swimming organs of the Sacculina are appendages moved by muscles seems obvious. As the young Sacculina needs considerable powers of locomotion if it is to have a good chance of finding a crab to which it can attach itself, it requires swimming organs more powerful than those provided by cilia. Why does it hatch out in so minute a form? Because it has to obey the maxim "you must cut your coat according to your cloth." The egg has but little food stored within it. The problem to be solved is, given a very limited supply of food, to produce a larva having considerable powers of locomotion. The solution is the type of larva that leaves the egg—a microscopic creature having an oval unsegmented body, provided with a single eye, a mouth, digestive system and three pairs of appendages, the front pair to assist the creature in feeding and the other two to serve as locomotive organs. It is difficult to conceive of an organism more simple than this having these characters. The larva must have some form and what more reasonable than that this form should be similar to that which proves to be so effective for larval crustacea? Crustacean larvae need considerable locomotive powers and this explains why they, like Sacculina, are not given cilia as swimming organs. On account of the outward resemblance of the larva of Sacculina to that of a crustacean, the name, nauplius

given to the larva of the latter is also applied to that of Sacculina, on a similar principle to that under which the visceral arches of the amniote embryo are called gill arches!

Before the larva can adopt a sessile life, it has to develop an organ enabling it to pierce the integument of its future host, means of attaching itself to and of obtaining nourishment from its host. While these organs are developing the larva has to seek its food in the water and protect itself against enemies. This being so, it is no matter for surprise that it develops a bivalve shell open below to give its swimmerets free play. This is called the Cypris stage because the larva in this condition bears a superficial resemblance to the Ostracode Cypris.

The Sacculina larva passes through these stages because either it is forced to recapitulate adult stages of its ancestors, or, evolution or no evolution, these successive conditions are necessary to its existence. Dr. Davies admits that "this is not actually a case of recapitulation." He asserts "Sacculina must have had ordinary non-parasitic Cirripedes in its ancestry; it only goes through the larval stages, not the adult stage of ordinary Cirripedes."

Why MUST Sacculina have had non-parasitic Cirripedes in its ancestry? If it had them, why must it pass through their larval but not their adult stages? Why is the supposed biogenetic law observed in the one case and disobeyed in the other? I contend that Sacculina had neither nauplius-like nor Cirripede ancestors, that it passes through its larval stages because these are essential to its development.

For my part, in default of experimental proof that

it is possible, I do not believe that any animal enjoying a free-living existence, ever became so completely sessile as to lose by degrees its eye and swimming organs. Be it noted that before the supposed free-swimming ancestor can have switched over to a sessile existence it must have developed the means of piercing the integument of the crab, of attaching itself permanently to it and of obtaining nourishment from its tissues, that is the capacity to develop long root-like processes able to absord and convey to Sacculina nourishing matter from the tissues of the crab. In this connection we must bear in mind that the evolutionist has to believe that this curious change of mode of living has occurred not only in the ancestors of Sacculina but in those of dozens of other kinds of parasites.

The curious animals, popularly known as sea-squirts, which look rather like double-necked leather bottles, furnish another example of metamorphosis. The scientific name of the group of which they are members is Ascidia or Tunicata. The larva is often called a tadpole because of its superficial resemblance to the tadpole of a frog. In the words of Professor W. Garstang "the whole of the embryonic processes lead directly by the nearest route to the building up of the larva," which attains a length of about $\frac{1}{4}$ inch. Four-fifths of this is composed of the tadpole-like tail, which is the organ of locomotion. In front of the body are three peculiar warts which secrete a sticky substance. These are the means by which the larva, after swimming about for some time, attaches itself to some inanimate object. It then undergoes complete metamorphosis. The tail, "notochord," nerve tubes and sensory vesicle are devoured by phagocytes formed within the animal, a

new set of organs is developed and the saclike adult is formed, which looks more like a plant than an animal. I submit that a transformation such as this cannot have developed gradually during a period of thousands of years, because during the long period of transformation lasting from the moment it had fixed itself until the new set of organs had developed the animal could have obtained no food except from its own tail. Why does the Ascidian larva develop as an organ of locomotion, not cilia or swimming legs but a great tail? Because the tail, after having served its purpose as an organ of locomotion, provides the animal with the food it must have after it has fixed itself and before the new feeding organs are developed.

In the same way the tail of the frog tadpole provides the latter with a store of food on which it can draw during the period in which its jaws are being transformed into the very different jaws of the frog.

To class Sacculina among the Crustacea and the Sea-squirts along with the vertebrates, as modern systematists insist on doing, tends to bring Zoology into disrepute. Why do they not class the frog as a fish?

The flat-fishes, i.e. soles, plaice, flounders and turbot, etc., undergo a limited metamorphosis, quite unlike that of Sacculina or of the Sea-squirt. The fish when adult have both eyes on the same side of the head, and swim about and rest with one side of the body facing upwards and the other downwards. More than 600 species of these flat fishes have been described. Some consider them to be an order composed of five families.

When it emerges from the egg, the flatfish has a symmetrical body shaped like that of an ordinary fish, with an eye on each side of the head. As it grows the body

becomes laterally compressed and the cartilage of the supraorbital bar above one eye (the left in some species, the right in others) becomes absorbed, leaving a gap through which the eye below will pass during its migration round the top of the head to the opposite side. The fish now begins to tilt over to the side from which the eye is moving. The migration of the eye is a matter of a few days. When the migrating eye reaches the supraorbital cartilage on the opposite side of the head, the other eye begins to move, and the two eyes travel together until they reach the position they occupy in the adult. Their combined movement causes the intervening supraorbital bar to become distorted. Then ossification sets in.

In this connection it may be noticed that the turning over of the flatfish to one side does not seem to be a necessary consequence of the flattening of the body, because some fish equally flat, such as the John Dory and the fresh-water fish, Pterophyllum scalare, remain vertical. Moreover, the Bream, which does not live on its side, is more flat than the Halibut, which is the least flattened of the flatfish.

The evolutionist apparently believes that some ancestor, or ancestors, of the flat-fish became rather flat and then, for some unknown reason the supraorbital bar on one side conveniently became absorbed, and the eye below began to shift its position, and this shifting became greater in successive generations, until eventually both eyes were situated on the same side; that is for thousands of years these fish swam about in a position intermediate between the vertical and the horizontal.

The fossils afford no support to this theory, the earliest known turbot, which occurs in the Eocene, and

the earliest sole, found in the Upper Miocene, are as flat as are any living flat-fish. All living fish assume in the water a perfectly vertical or a perfectly horizontal position; none of them swim with a list, as evolutionists suppose many kinds of fish to have done in the past.

The metamorphosis of a caterpillar into a butterfly is a process which cannot have developed gradually step by step, generation after generation. Before undergoing metamorphosis the caterpillar has to manufacture a protective covering. This wonderful feat and those that follow are graphically described by Mr. E. L. Grant Watson in his "Enigmas of Natural History." As I have not watched the process I take the liberty of quoting his account of the transformations effected after the caterpillar has entered the pupal stage (p. 72): "The shape and position of the organs of the butterfly which is to be, are already stamped on the pupa. It should be particularly noticed that these marks are on the OUTSIDE and that there is nothing yet formed inside to correspond with them. This is a significant fact and one which, when its significance is grasped, will modify the accepted idea that development takes place always, and only from a centre outwards. The governing idea has at this stage at last declared itself, and although there is within the creature, at this stage, nothing but a green watery pulp, all the places in its organism which are later to be occupied by legs, wings, antennae, etc., are now definitely marked. They are waiting to be filled by the organs, not yet made, but already determined..... Later a new generation of tissue is formed, partly from this central intestinal magma, and partly from the proliferation of special corpuscles called image-bearing discs Thus it is that

the newly-formed portions seem to have no direct filiation with the destroyed parts of the larval organism. The creature has in fact died, in so far as it has lost its form, its organs, and its habits, and now, in a manner which cannot be described as anything but mysterious, is experiencing a new orientation towards a quite different form, which is to find its expression in quite a different mode of life."

Does any one really believe that the ancestors of butterflies were as adults just masses of pulp enveloped in cases, having no means of procuring external nourishment? If not, it is for the evolutionist to explain how the process of metamorphosis became intercalated in the life-history of the caterpillar. The caterpillar has biting jaws by means of which it rapidly demolishes the leaves on which it subsists; on the other hand, the mouthparts of the butterfly are entirely different: the butterfly is incapable of biting anything, its first maxillae are grooved and greatly elongated, often coiled; pressed together they form a tube through which nectar from a flower is drawn into the stomach, which is a suction pump. The transformation of mouth parts of the caterpillar type to those of the butterfly type involves a period during which the possessor of the mouth has either to go without food or subsist on its own tissues. Thus it is impossible that such transformation was effected in the past by a gradual process extending over a period of thousands of years. Unless the transformation was effected suddenly and miraculously butterflies must from their origin always have had suctorial mouthparts.

Nor are these the only difficulties presented to the evolution theory by the phenomena of metamorphosis.

As all kinds of larvae—caterpillar, nauplius, trochosphere, etc., are able to feed and fend for themselves; the evolutionist has to explain why these supposed ancestral forms have ever developed into anything else. Why, for example, do not caterpillars breed as caterpillars, and reproduce themselves as such? Why do we not see around us adult animals in the form of caterpillars or tadpoles, some of which are beginning to develop into modified forms, i.e. incipient moths or butterflies and incipient frogs or sea-squirts?

Endoparasites, which in the course of their life change their hosts, furnish difficulties for the evolutionist quite as formidable as any of those noticed above.

Consider the liver-fluke, Distomum hepaticum. This spends its adult life in the intestine of a sheep. The eggs after being laid pass out of the sheep into the open where they hatch out as little ciliated embryos that swim about in water. In order to survive, these larvae must find a pond snail (Limnaea truncatula.) Having reached the snail the larva finds its way into the pulmonary chamber. Here it loses its cilia, increases in size and buds off germinal cells into its own body-cavity. While in this condition it is known as a sporocyst. The buds it casts off develop into secondary larvae known as rediae; these are elongated oval in shape, have a mouth and stomach, and are provided, on the under side, with a pair of knob-like processes by the aid of which they move. They force their way out of the sporocyst and enter the tissues of the snail where they give birth to other rediae that develop into larvae known as cercariae, which in outward appearance resemble broad-bodied tadpoles, and even exhibit a "notochord." Using the long tail as an organ of loco-

motion, the cercariae work their way out of the snail, then attach themselves to blades of grass where each sheds its tail and encases itself in a tough sheath. They remain in this condition until they happen to be eaten by a sheep when grazing. Inside the sheep they find their way into its liver where they develop into the adult state and acquire sexual organs. They then migrate into the intestine of the sheep and thus complete the cycle of their life history.

Notice that in the earliest part of its life the liver-fluke has to be adapted to life in the open, next to existence in the pulmonary chamber of a snail, then to living inside its tissues, after that once more to life in the open, then in the stomach of the sheep and next in its liver, and finally in its intestine. Failure of the animal to accommodate itself to any of this succession of conditions would lead to the rapid extermination of the species. Moreover the liver-fluke is not the only species to enjoy such a variety of environments. Hundreds of other species lead a similar existence; in some the variety of environments is even greater. Thus the tape-worm, Diphyllobothrium latum, spends part of its life in a copepod, part in a fish and part inside a human being.

Another important point is the immense variety of these parasites. The Trematoda—the Class to which the liver-fluke belongs—are all parasites; about sixty different families of these are endoparasites, i.e. parasites which live inside their hosts, and 13 families are ectoparasites, those that live outside their host. That is to say this Class of parasites is more diversified than is the Class that includes all the mammals. Another Class —the Cestodes,—of which all the members are endoparasites, is composed of five Orders embracing 30

families. The evolutionist has to explain not only how parasitism arose but why these parasites, both internal and external, have become so diversified. It is not difficult to believe that what appear to be different species of the same genus might result from some members of the genus living in one kind of tissue and others in another; thus the parasite Schistosoma which causes the disease known as bilharziasis in man, assumes the form known as S. haematobium when it lives in the bladder and S. mansoni when it lives in the large intestine. But how can evolution have caused such diversification as to lead to the origin of new genera, families and orders? In this connection it may happen that parasites belonging to different Orders may occur in the same type of host.

The above are difficulties presented by one aspect of parasitism. Those presented by other aspects are equally formidable. A whole volume could be filled with examples furnished by insect parasites. Let us notice one instance as showing the nature of these difficulties.

The hymenopter, Ibalia, victimises the wood-wasp, Sirex. The latter bores with its powerful ovipositors a hole in the trunk of a conifer for the reception of its egg. This last yields a grub which feeds on wood. The grub when feeding bores a tunnel in the wood, in which it lives for some years until it turns into a grub that develops into the adult wasp which, by the aid of its powerful jaws, bites its way out of the tree. How this particular habit gradually developed is a difficulty of the evolution theory which we cite in passing. We are here concerned with the habits of Ibalia. This, using the hole in the trunk already bored by Sirex, lays its egg inside the Sirex grub. On being hatched the Ibalia

larva feeds on the tissues of the Sirex, not devouring the vital organs until it has eaten the others, thus obtaining all along fresh meat. Ordinarily the Sirex grub bores deep, but when the Ibalia is inside it, it changes its habit and burrows towards the surface, with the result that Ibalia, of which the jaws are not powerful, has not to bore far in order to get out of the tree trunk. If the Ibalia grub were to devour the vital organs of its victim too soon it would starve to death, and it would perish inside the tree did not the Sirex grub change the direction of its boring. It is conceivable that natural selection might have fixed the habit of the Ibalia larva to attack the vital organs last, by weeding out all who did not do this, but how could natural selection have originated in Sirex the change in its boring habit, from which it derives no benefit?

The books of many entomologists, particularly those of Henri Fabre, are full of examples of habits difficult to account for on any theory of evolution.

CHAPTER VIII.

Nascent and Vestigial Organs.

I asserted (D. p. 24) that the theory of evolution requires for its proof, not vestigial, but nascent organs, because the existence of useless vestiges merely shows that animals may lose organs. I wrote: "although the anatomy of thousands of species has been carefully studied, it is impossible to adduce a single structure in any species which is indubitably or even probably in a nascent condition." This is clearly a very great difficulty of the evolution theory, I might almost say a fatal one. Nevertheless Dr. Davies neither challenges these statements nor attempts to reply to them. The word "nascent" does not occur in his book!

Consider the significance of this absence of nascent organs.

According to the evolution theory all multicellular animals are derived from one-celled ancestors, which exhibit nothing that can be called an organ in the strict sense. Consider now the vast number of organs and structures which are supposed to have evolved in the descendants of these organ-less ancestors; every differentiated cell, bone, cartilage, muscle, tendon, nerve, blood vessel, ganglion, hair, feather, scale, spine, shell, spur, antler, horn, hoof, claw, nail, tooth, tusk, antenna, appendage, every internal organ from the blood corpuscles to the stomach and liver. Every type of each of the

above organs, according to the evolution theory, must have at one time existed in a nascent condition. Now consider the million or so existing species of animals all of which are supposed to be in a state of flux, evolving. If these species be really evolving, the majority of them ought to exhibit nascent structures in all states of completion, from unrecognisable excrescences to structures almost ready for use. Not a single one seems to exist! I know of only four explanations of this lack of nascent organs. 1. Every type has been separately created with all its organs. 2. New structures arise in animals, not gradually, but *per saltum*. 3. Evolution no longer operates: it is a thing of the past. This appears to be the view of Professor Broom, who writes ("The Coming of Man" (1933) p. 225): "As nearly all the great changes in evolution, except the evolution of man, took place before the Miocene Age, and very little evolution of any sort has taken place since the Pleistocene, i.e. in the last million years or so, it looks as if the agencies that directed evolution are no longer active on earth, or at least that their activities are different. Possibly they are no longer interested in bodily evolution, but engaged in the more important work of evolving higher types of humanity." 4. That the capacity of evolving has ceased in the vast majority of animals, but is the possession of a few of them. This is held by Professor Cuénot, who writes ("L'Adaptation" (1925) p. 375): "I do not mean. . . .that evolution has definitely ended on the earth, and that we know all the possible formulae of life. . .I readily believe that there is still a reserve capable of giving rise to new beings quite unlike anything that existed formerly, or exist to-day. I look for these in microscopic families of uncertain affinity, dis-

regarded by almost all zoological treatises, marine or freshwater organisms such as the Echiuroidea, Tardigrades, Dinoflagellates, Archiannelids, Rhodope, terrestrial forms such as Myrientomata."

The first of the above explanations is pure creationism, the second is creationist, rather than evolutionist, while the last two are evolutionist. They account for the failure of practical breeders and geneticists to produce a new type of animal. Broom's views cannot be tested experimentally, those of Cuénot can. I do not anticipate that breeding experiments on any of the animals cited by Cuénot will produce any new types, because his theory is in direct conflict with the geological record, which indicates that no new phylum has come into being since, at any rate the Ordovician Period. Professor Broom's idea that the agencies which direct evolution are now concentrating on higher types of humanity is not borne out by Archaeology or pre-history. There is no evidence that human nature is changing.

I regard the theories of Professors Broom and Cuénot as valiant attemps to save the face of evolutionists, but they do not overcome the difficulty presented by the absence of nascent organs, because, so far as I am aware, no fossil exhibits a nascent organ: the earliest known fins are fully developed, so are the earliest legs, and wings, whether of insect, bird, bat or pteradactyl.

Although Dr. Davies has nothing to say regarding nascent organs, he makes gallant efforts to enlist vestigial organs as witnesses for evolution. I do not agree with his statement (E. p. 166): "The existence in any animal of structures to which no use can be assigned, but which are obviously identical with structures that are useful in other animals, has always been a fact easier to

reconcile with evolution than with creation." The existence of such structures is certainly easier to reconcile with a theory that structures may in time degenerate than with a theory that this cannot happen; but the latter is not necessarily part of a theory of special creation. Thus, if it could be shown (which it cannot) that the wisdom teeth of man are disappearing and are now useless relics, this would not effect the theory that man was specially created.

Admittedly, the existence of a large number of useless organs that are not the result of the way embryos develop, i.e. are not embryonic remains, would indicate that many organs have degenerated. But the truth is that useless structures, fulfilling these conditions, are very rare. At the time I wrote "Difficulties of the Evolution Theory" I had not discovered that some structures commonly asserted to be useless and so truly vestigial are nothing of the sort, e.g. the splint bones of horse, lateral toes of even-toed ungulates, the teeth in the foetus of toothless whales, and almost certainly the vermiform appendix of man. As to the wings of struthious and other wingless birds, there is no evidence that their ancestors possessed bigger wings. The eyes of some animals that dwell in dark caverns and the stumps of wings seen in some flightless insects should, I think, be described as undeveloped organs rather than as vestiges. Let us now consider each of the above.

1. *The splint bones of the horse.* These, as Hayes points out, (1) strengthen the leg, (2) serve as an attachment for certain muscles, (3) in conjunction with the canon bone form a groove in which lies the upper part of the suspensory ligament,—an elastic brace supporting the fetlock and counteracting the effects of

weight.

Several other considerations indicate that the splint bones of the horse are not useless vestiges. There is no evidence that they are diminishing in size. Many mammals have a collar bone, while in others there is no trace of this bone. If these latter have lost this bone because it is of no use to them, why have horses not managed to rid themselves of the whole of digits nos. 2 and 4?

2. *The lateral toes of deer and other artiodactyls.* In the horse the splint bones represent the proximal or upper part of the metapodials; evolutionists suppose the distal or far end to have been lost. In deer, however, the lateral toes represent the far ends of the metapodials with the attached toes; in them the proximal ends are supposed to have disappeared. Those who believe the lateral toes in both deer and horse to be useless vestiges have to explain why the former have lost the proximal portion and the horse the distal. Clearly these supposed vestiges serve a purpose which differs in the two types. We have noticed the uses of the splint bones of the horse. The position and degree of development of the lateral toes in deer varies with the species, or genus. Each kind of deer has the type of foot needed to prevent its feet sinking deep into soft ground. At one extreme is the lightly-built, small-antlered Cervulus, of which the lateral digits lack toe-bones, at the other extreme are the musk deer and reindeer, of which the lateral toes are nearly as large as the middle pair, in consequence the foot presents a broad surface to the ground and the lateral toes prevent their possessor from slipping on a frozen or rocky surface.

In the pig the fifth toe seems to be represented by the trapezoid to which a ligament and a muscle are at-

tached. The side toes are useful in marshes and on steep slopes; the hoofs on these get worn, as do those of the middle pair.

In the ox tribe the lateral toes are wanting in some; in most they are represented by a hoof only or by a hoof and nodules of bone. In the ox the lateral toes take the form of horny knobs known as ergots, provided with ligaments to prevent them being pushed up and down when the leg is extended. They support the toes when these receive shocks from below or from one side on rugged ground; they are also serviceable in marshes and on steep slopes. As the ox, unlike the boar, is not addicted to mountains, there is no need for its ergots to be regular hoofs. Each species has the type of foot best suited to its requirements.

3. *The teeth in the foetus of toothless whales.* When I cited these as useless vestiges I had not read Vialleton's " L'Origine des Etres vivants," where he writes (p. 164) : "Certain of these (supposed vestigial organs) deserve a special examination because they play a part that escaped the notice of Darwin. When he cited as truly vestigial organs the germs of teeth in the foetus of whales devoid of teeth in the adult state, and those of the upper incisors of certain ruminants, the gums of which they never pierce, he forgot that these germs in mammals, where they are very large relatively to the parts enclosing them, play a very important part in the formation of the bones of the jaws, to which they furnish a *point d'appui* on which these mould themselves. Thus these germs have a function."

That Darwin was wrong and Vialleton right is indicated by the following facts :

(a) To quote Vialleton, "the disposition (of these

foetal teeth), their form and their number, different from those of other Cetacea, show that in the whalebone whale, far from being merely the relics of an extinct ancestor, they have an individuality and a causality peculiar to them, since they are multiplied and adapted to the length of the jaw."

(b) It is highly improbable that the ancestors of the toothless whales first acquired a number of additional teeth, then scrapped them all and developed in their place the extraordinary baleen plates that occur in the mouth.

(c) No living or any known Tertiary bird has teeth, but Archaeopteryx and all known Cretaceous birds had well-developed teeth. If, as the evolutionist supposes, modern birds are derived from toothed ancestors, many, if not all, birds should exhibit foetal teeth, as whalebone whales do, but no known bird embryo shows any trace of teeth. The supposed rudimentary teeth that have been described in parrots, are not teeth but papillae, similar to those under the hoof of the horse, which provide horny tissue to make good that worn away. Birds lack embryonic teeth because these are not necessary for the moulding of the very slender jaw.

(d) American Ant-eaters lack teeth, and having attenuated bird-like jaws, no teeth appear in the foetus, although embryonic teeth occur in the toothless Edentata of which the jaw is comparatively massive.

(e) In the adult both the known Monotremes, Platypus and Echidna, lack teeth, but while the embryo of the slender-jawed Echidna shows no teeth that of Platypus does, and these persist for some time.

4. *The Vermiform Appendix in Man.* Birmingham seems to have been the first to suggest that this is not

a useless organ. Having discussed this matter elsewhere (M. p. 75) I will here merely mention that Birmingham's view is supported by the fact that the appendix is richly supplied with blood, and has a complex wall composed of an external muscular coat, followed by a mucous layer, under which is a mass of lymphoid tissue which DOES NOT APPEAR UNTIL AFTER BIRTH, and finally an inner coat of mucous membrane. The use of this organ would probably have been discovered ere this, but for the fact that useless organs, being required as evidence for evolution, are eagerly sought after by transformists!

5. *Blind Cave dwellers.* Numerous animals, including tailed amphibia, fish, insects and spiders, that inhabit dark caverns are blind. Some of these have traces of eyes; in others the eyes are degenerate. It is doubtful whether imperfect eyes in such cases are the result of evolution, because:

(a) In some species of beetle the males have fairly large eyes, while these organs are much reduced or lacking in the females.

(b) The amount of degeneration of the eye varies greatly in individuals of the same species. Thus, the spider, Troglohyphantes, may be quite blind in one cave and have small eyes in another. McIndoo found in one cavern a species of spider of which the individuals exhibited all degrees of eye degeneration, the number of eyes varying from 8 to 0. Racovitza examined 59 individuals of the Isopod, Trichoniscus gachassini, living in a cave near Algiers and found that 2 near the entrance had small but well-developed eyes, 36 had very degenerate eyes and 21 had no traces of them.

(c) Some animals inhabiting dark caves have perfect-

ly-developed eyes.

(d) The eyes of the blind amphibian, Proteus, normally do not reach the darkened skin, but its larvae, if exposed to red light, develop normal eyes.

(e) Sexton and Winge, when experimenting on the shrimp, Gammarus chevreuxi, which has black eyes, produced suddenly in broad daylight, forms having red eyes, those with white and those having degenerate eyes. Similar results were obtained in the case of the fruit fly, Drosophila melanogaster.

(f) From abnormal eyeless forms thus produced, individuals having normal eyes may be bred.

(g) Payne raised 69 generations of Drosophila melanogaster in complete darkness without effecting any change in the eyes or in the desire to seek light.

It would thus seem, that, in some circumstances, the gene complex believed to control the development of the eyes does not work normally. Should blind mutants appear in nature these would soon be wiped out, but, if this happened in a dark cave, there is no apparent reason why blind individuals should not survive to produce offspring.

6. *Flightless insects on Islands.* A. R. Wallace wrote ("Darwinism" p. 105) of the insects of Madeira: "Many have either lost their wings or have had them so reduced as to be useless for flight, while the very same species on the continent of Europe have fully developed wings.....The explanation of this change is that Madeira.....is much exposed to sudden gales of wind, and, as most of the fertile land is on the coast, insects which flew much would be very liable to be blown out to sea and lost. Year after year, therefore, those individuals which had shorter wings, or those

which used them least, were preserved; and thus, in time, terrestrial, wingless, or imperfectly winged races or species have been produced." This hypothetical explanation assumes that the smallest change in the size of a wing may be a matter of life or death. Far more probably the case of these wingless or stunted-winged insects is on a par with that of blind or partially blind cave dwellers. Professor T. H. Morgan and his collaborators, when breeding the fly, Drosophila melanogaster, found that individuals occasionally crop up of which the wings are defective, stumpy, vestigial or entirely lacking. This may well have happened in Madeira. In ordinary circumstances a mutant that could not fly would soon perish, but on Madeira, where gales are frequent, winglessness might be beneficial in some cases. Are the wings of these Madeira insects lost or merely in abeyance? If the former be the case this would be proof that animals may lose structures, and if the evolutionist be content to limit the theory of evolution to loss or decay of structures, he will have made out his case.

Having disposed of the structures, which at one time I believed to be useless vestiges, let me now deal with three structures that I classed as embryonic remains (D. p. 28 and 55—57) i.e. useless to the adult, but the necessary results of the manner in which embryos develop, because I did not then realise their usefulness to the adult organism, viz. the semilunar fold of man's eye, the muscles of his external ear and his hidden tail.

The semi-lunar fold. Every higher embryo exhibits the primordia that give rise to the upper and lower eyelids and another which may give rise to a third eyelid, as in many birds, or to the semilunar fold. As this

regulates the flow of the lachrymal fluid, it is not a mere embryonic remainder.

The muscles of the External Ear. Dr. Davies evidently accepts Darwin's view that these are relics of the panniculus carnosus which extended over great part of the body of remote ancestors of man. This muscle system enables its possessor to twitch the skin and thus obtain some relief from insects that settle on it. The human skin, owing to the shortness and fineness of its hairs in most parts, is peculiarly exposed to insect bites, as all who have lived in the tropics are aware. This being so, in default of very strong evidence that man's ancestors possessed this useful system of muscles, I cannot believe that the greater part of it has been lost. As muscles are very variable and as some birds have an ambiens muscle while others show no trace of this, it is highly improbable that man should retain any part of a useless muscle. Thus, there are *a priori* grounds for the belief that these ear muscles of man serve a useful purpose. Vialleton writes (O. p. 163): "It should not be forgotten that an organ always presents in the various animals a large number of different degrees of complication which it is possible to arrange in a series, of which the extreme terms are widely separated from one another, without, however, conveying the right to consider the most simple of them as vestigial in the transformist sense. To regard them as such is to go a little too fast; for example our auricular muscles, on the pretext that the external ear can be moved only by certain individuals; for these muscles, without moving the ear, may serve to stretch or fix the epicranial aponeurosis. There is great difference between the action of a muscle and that of a simple contractile cord having the

same attachments as the former. Every muscle, in effect, produces at the same time as its principal action, an infinity of little secondary movements on its enveloping aponeurosis, on the connective tissue and on the vessels that surround it. It is all this, together with the action of antagonistic muscles, which harmonises the movements and makes them different from those of a dancing Jack, and this is why the principal action may be lacking and the others remain necessary."

The hidden bony Tail of Man. In one sense this is an embryonic remainder, because an external tail is necessary for the development of the human embryo, but, as this tail, after being withdrawn within the body, has its use (see p. 37), it is not strictly correct to describe it as a purely embryonic remainder.

Embryonic Remains. From the foregoing it is clear that the above structures must be deleted from the list of embryonic remains. (D. p. 28). I have in my earlier book (D. p. 55) shown the true significance of these embryonic remains.

We have now to consider the examples of vestigial structures cited by Dr. Davies (E. p. 168), viz. the tiny shells of some molluscs.

He deems the shell of the slug Testacella a useless vestige of a well-developed ancestral shell, and apparently regards the shell as one of the ills that afflict molluscs, of which many of them are endeavouring to rid themselves, with various degrees of success, the Nudibranchiata having achieved complete success.

In several groups of Gastropods the shells of different species exhibit great differences in size. Does this fact convey the right to assume that the small shells are the result of atrophy? I doubt it. It is difficult to fur-

nish proof, because shell-less animals are rarely fossilised: but, as the genus Bulla, mentioned by Dr. Davies, has been found fossil in early Tertiary deposits, if the shell be undergoing atrophy, the process is in this case exceedingly slow.

The shell is a valuable protection to a soft-bodied animal. The best protection is afforded by a shell large enough to accommodate the entire body of its possessor; but such a shell necessitates sluggish habits and is not suitable for an active animal. The smaller the shell the less the protection it affords, but the greater the mobility it allows. A mollusc cannot have it both ways. If it lead an active life it must be content with a small shell, or none at all. Molluscs devoid of a shell or having a very small one, need other means of defence. The sea hare (Aplysia), cited by Dr. Davies, is able to eject a concealing cloud of purple fluid, and is unpalatable. Other naked species protect themselves by secreting acids, or appropriating the nematocysts or stinging cells of sea-anemonies. How they contrive to accomplish this feat is a mystery (see "Enigmas of Natural History," p. 8. by E. L. Grant Watson).

Some naked molluscs take refuge in sponges or behind sea-anemonies, others live in the discarded shells of other molluscs.

I do not assert that in no case has the shell of any mollusc undergone atrophy as the conditions of life changed, but there is no proof that this has happened. In my view it is probable that the shell, no matter how small, has some use. Professor Goodrich remarks (B. vol. 8): "It is doubtful whether any really useless parts are ever preserved for long unless they are insignificant."

From the foregoing it is apparent that the number of useless organs diminishes as our knowledge increases. May it not be that this number is the measure of our ignorance? Is not the day at hand when it will be impossible to name either a nascent or a vestigial structure?

CHAPTER IX.

Principles and Hypotheses.

Dr. Davies devotes an interesting chapter to what he describes as some leading and misleading principles of evolution. Let us consider these in turn.

1. *Cuvier's principle of correlation.* I agree that great discretion has to be employed when making use of this principle. One anatomist mistook a fossil tooth of a pig for that of an anthropoid ape, and another the tooth of an early type of horse for that of a monkey! Dacqué remarked that some of those who make restorations based on a few bones would be greatly surprised if they could see in the flesh the creatures they had reconstructed. The pictures printed in textbooks and the busts exhibited in museums of Heidelberg man, Piltdown man, Pithecanthropus, etc, are a disgrace to science. Nevertheless the principle of correlation is a sound one, because every animal is a well-constructed organism, of which the parts are in harmony. That is why fossils have not been found, and I venture to predict will never be found, of the pro-aves, pro-chiroptera, pro-cetacea, which most evolutionists believe to have existed in the past.

2. *The "principle" of recapitulation.* This I have dealt with in Chapter VI.

3. *The "principle" of change of function.* Probably most organs have functions subsidiary to their main

one; the failure to remember this has led to some useful structures being deemed useless vestiges. It may be that, with a change in conditions, a subsidiary function of a structure may become the most important. That this may lead to new developments in the organ in question is a supposition, of which, so far as I am aware, no proof exists. It certainly has not been proved that in man's ancestors the thyroid gland functioned as an endostyle. The fact that the bones supporting the uro-genital orifice in whales are derived from the primordia which give rise to the pelvis in quadrupeds is no justification for asserting that the ancestors of whales were quadrupeds.

Although the third and fourth visceral arches give rise to no structures in any existing amphibian, reptile or bird, Dr. Davies does not believe that, after giving rise to no adult organs for millions of years, these arches acquired the power of producing the epiglottis and part of the thyroid cartilage. He relies on the "principle" of change of function and thinks that these arches formerly gave rise to some structures, not described by him, that changed their functions and became the modern epiglottis and thyroid cartilage. This is a supposition which can neither be proved nor disproved.

4. *Parallel development, convergence and adaptive radiation.* These are names invented to bring certain facts into line with the evolution theory.

If all placental mammals be descended from a common ancestor, it follows that the descendants of this ancestor have assumed a great variety of forms, in the words of the evolutionist they have undergone adaptive radiation. This applies equally to the marsupials, and, as some of these resemble more or less closely some

placentals (E. p. 153). the animals showing these resemblances are said to have undergone parallel evolution.

Sometimes organisms which do not seem to be at all nearly related resemble one another fairly closely. Thus, as Dr. Davies has shown, a variety of animals have prehensile tails; these tails are said to have arisen by converging evolution.

But, as Dr. Davies admits, divergent evolution must precede convergent evolution; this double process might be called zig-zag evolution.

Again evolutionists think, in the words of Dr. Davies (E. p. 142): "that characters originating as adaptations to embryonic or larval conditions may afterwards continue in the adult stage." This supposed process is called clandestine evolution by Dr. de Beer.

There is yet another kind of evolution, not mentioned by Dr. Davies. In my "Man: A Special Creation" I suggested that it might be styled disorderly evolution. This type of evolution has been invented because a comparison of the anatomical characters shared by the animals of any big group give the most contradictory results if used as a test of evolution. In the case of birds I dealt with this elsewhere (D. pp. 164-175). Of the Primates Sir Arthur Keith writes; "From the details revealed by anatomical analysis it is plain that evolution has not proceeded in an orderly or simple manner in shaping the bodies of the higher Primates; characters are curiously scattered." ("Man" B. vol. 14, p. 761). Clearly then Evolution is a name to conjure with!

5. *The "principle" of change of habit.* This "principle" is introduced, somewhat surreptitiously, by Dr.

Davies in his reply to my question (D. p. 138) : Does it not seem strange that, having accomplished the feat of changing from an amphibian to a reptilian mode of life, half a dozen orders of reptiles should have returned to the liquid element? "Not at all strange" replies Dr. Davies (E. p. 154), "if we consider that a successful group soon fills up its habitat and the pressure of population drives its marginal members to venture into other habitats."

Apart from the fact that it is very doubtful whether any species breeds up to the margin of subsistence the idea that, as a consequence of pressure of population, a terrestrial animal became converted into an aquatic one is, in my opinion, fantastic.

In support of this idea Dr. Davies points out that the opossum, Didelphys azarae, although well adapted to an arboreal life, lives in a treeless habitat and so seems to have changed its habits. I find no difficulty in believing that, should a locality become deforested, some of the arboreal inhabitants might contrive to live on the ground, but, had this area been invaded by the sea, I decline to believe that D. azarae would have been converted into a seal-like creature. Nevertheless, according to many evolutionists, the change from aquatic to terrestrial existence and *vice versa* has happened very often; thus Miall writes ("Natural History of Aquatic Insects" p. 5) : "I think we can say with a considerable degree of probability that the change of habitat from terrestrial to aquatic has taken place in the Class of insects at least a hundred times quite independently, and the number may be much greater than a hundred."

6. *Irreversibility.* Dr. Davies cites with approval Dollo's Law; evolution is an irreversible process; an

organism cannot revert even partially to a condition already passed through in the series of its ancestors.

This theory is diametrically opposed to the theory of atavism or reversion about which Dr. Davies says nothing. Very rarely a horse is born having three toes on its front or hind feet. Evolutionists deem this to be a throw-back to a three-toed ancestor. If the horse be descended from a three-toed ancestor this is clearly a case of reversed evolution. We do not know what causes the additional toes thus to appear, but if this happened to many wild horses in any locality, it is conceivable that a three-toed race might arise.

It is impossible to reverse a process that has not occurred. If then, as the data show, there has been no reversal of evolution, may not this mean that evolution has not taken place? That this is the correct interpretation is rendered probable by the fact that there is evidence that changes proved to have taken place in organisms are reversible.

Professor L. S. Berg found (N. p. 229) that the fossil fish, Myxocephalus quadricornis, in the Yoldian of the Baltic had four horny excrescences on the head, and that in the Ancylus epoch, when the Baltic Sea became a fresh-water lake, the species lost its horns, but regained them when the sea again became salt. To-day this fish has these horns in the sea but not in fresh water. Analagous changes occur in the crustaceans, Limnocalanus grimaldii and Mysis oculata, in the Baltic Sea. We see a similar phenomenon in the case of deer (see p. 156) and probably in the mollusc Planorbis. (see p. 154)

Let us now notice Dr. Davies' examples of irreversibility.

Sharks have gill openings on the side of the throat, the dorso-ventrally flattened skates have the openings on the ventral surface, as have the saw-fish, although their body is not flattened. Dr. Davies assumes that the skates are descended from sharks and saw-fish from skates. There is no fossil proof of this; indeed the fossils at present known are unfavourable to it. The skates date back to the Jurassic, whereas Sir A. Smith Woodward writes (" Guide to fossil Reptiles, Amphibia and Fishes in the British Museum (Natural History)", p. 40) : "The spiny dog-fishes...seem most nearly to represent the ancestors of the...skates, but they are only known to date back to the Cretaceous period."

Similarly there is no evidence that the tree-kangaroos are derived from five-toed ancestors, or *Ichthyosauruses from land reptiles.

The uncoiling of the Cephalopods seems to be a case of reaction to unfavourable conditions. (see p. 187).

7. *Unequal rates of evolution and persistent types.* Among the difficulties of the evolution theory cited by me (D. p. 81) are the great differences in the span of life of species of the same Class and the differences in the length of time during which genera and larger groups have persisted, judging by their known fossils. Thus, there is a record of a parrot having lived for 115 years, while the age limit of the pheasant appears to be about 15 years. Several genera have persisted from the Ordovician, while others, judged by the known fossils, seem to have lasted less than one geological period. Dr. Davies has not dealt with the first difficulty. As regards the second, he suggests two alternative explanations of the long persistence of the Brachiopod, Lingula.

* Ichthyosauruses have more than five digits i.e. more than any known land vertebrate.

The first is that the fossils described as Lingula may not all belong to the same genus; the Lingula type may have arisen repeatedly, as the Gryphaea type of oyster seems to have done (see p. 185). The other explanation is that, as Lingula is a burrowing animal, the conditions of its life are so common and constant that they can never have been wanting at any time, and, as it subsists on microscopic food, any variation in this can hardly have had any selective value. There is much force in these remarks, but, as there are many kinds of burrowing animals, if this be the whole explanation a far greater number of Ordovician genera ought to have persisted until the present day. I am inclined to think that Lingula has persisted so long in shallow seas because it is able to endure a great range of temperature.

There is geological evidence indicating that in parts of the earth the climate has undergone great changes. During most of the Tertiary Period the Arctic regions enjoyed a mild climate, as did the Antarctic regions in the Jurassic. On the other hand, Northern Europe and North America appear to have been ice-bound during the Pleistocene, as were South Africa, Central India and Australia during the late Carboniferous or early Permian. Such climatic changes must, evolution or no evolution, have resulted in the replacement of the local floras and faunas. The fossils show that every locality has been inhabited by a succession of floras and faunas, and that the later ones are not descended from those that immediately preceded them in the locality, but were either immigrants or new creations. Thus, Lingula would seem to have been subjected to a great variety of climatic conditions since the Cambrian Period.

Dr. Davies says that the critics of Evolution often

ask: where are the living species to-day that mark the beginning of new families, orders and classes? I put the question differently; I ask: where are the nascent organs in all stages of development that ought to occur in many organisms if the theory of organic evolution be true? To this question I await a reply.

8. *Non-adaptive variation.* I do not think that Dr. Davies does justice to Paley. Paley was not concerned with the minutiae of the outward form of animals; he made no mention of coloration or markings on animals or their shells. He dealt exclusively with morphology and physiology, and demonstrated that every type of animal is endowed with precisely the organs it needs. As nothing was known in his day of the endocrine secretions he could but guess the uses of the ductless glands. He also laid stress on the variety of the organic world and went so far as to say ("Natural Theology" p. 170): that this might "induce us to believe that VARIETY itself, distinct from every other reason, was a motive in the mind of the Creator, or with the agents of His will." I know of no better explanation than this of the variety of the living world, a variety of which Paley was not fully aware, for in his day it was not known that apparently the whorls on the skin of the finger tips are not exactly alike in any two individuals. To my mind one of the most impressive phenomena of the organic world is this variety, coupled with the stability of the type. It would seem that in a sense every individual is a new creation. Non-adaptive variations are a most interesting study, and if they have any significance other than that of securing that no individual shall be a replica of another, the researches of such observers as Crampton and

Garrett, mentioned by Dr. Davies, may lead to the discovery of this.

9. *The Principle of Extravagant Extrapolation.* Dr. Davies makes no mention of the principle on which the theory of evolution is founded, the principle which Mr. J. Gomborow calls the "Pernicious Principle of Extravagant Extrapolation." The evolutionist observes that animals are variable and that from an original organism a number of varieties may be derived, and he forthwith extends almost indefinitely this observation; thus, because the various breeds of domestic pigeon are derived from the blue-rock pigeon, he extends the process and asserts that the blue rock pigeon is itself derived from an amoeba-like ancestor. A certain amount of extrapolation may be legitimate, but this, if scientific, should be confined to very narrow limits. "It certainly would be a great boon" writes Mr. Gomborow ("Why God and Where" p. 114), "if some liberal-minded scientist or scientists had the unprecedented courage to publish a set of science books, particularly on astronomy, geology and biology, telling us only what has actually been observed and ruthlessly eliminating everything that smacks in the least of extravagant extrapolation."

CHAPTER X.

Reptiles and Birds

In the chapter entitled "Reptiles and Birds," which contains much valuable and not easily accessible information, Dr. Davies puts questions to Creationists and criticises some observations made by me.

"We may" he writes (E. p. 190) "fairly ask the Creationist how he interprets this record (i.e. that of fossil birds). If he accepts it as approximately correct, he must infer that two species of land-living birds with reptilian tails were created (or their common ancestor created) in the late Jurassic period, and that the Class was then allowed to become extinct. Fifty or sixty million years later at least two new families of birds were created, with avian tails but with reptilian teeth, and one of them with useless wings, both adapted to a water-life. In the Palaeocene these Cretaceous birds had become extinct, but a number of new families were created, some equally doomed to early extinction, but some surviving to the present time (gulls, cranes, plovers), water birds still predominating. Then by gradual extinctions and new creations the bird fauna became more and more like that of to-day.

Why should the creation of birds during the Jurassic and Cretaceous periods have been so spasmotic and capricious, in contrast to the steady programme of the Tertiary? If the Creationist does not believe it was

spasmotic, then he must admit a very great imperfection in the record and cannot complain if the Evolutionist does the same."

To this I reply that in my former book I suggested that the record in the case of arboreal mammals is probably incomplete, and this applies *a fortiori* to flying birds. In my present volume I give reasons (p. 135) for my present belief that the record may be very incomplete in the case of land organisms until near the end of the Mesozoic Period.

Without admitting (see p. 106) that anything approaching fifty or sixty million years elapsed between the laying down the fossil of Archaeopteryx on the one hand and those of Hesperornis and Ichthyornis on the other, I think it probable that birds were in existence during the interval and earlier. I think it highly probable that the earliest known fossils of each family of birds represent immigrants to the locality in which their fossils occur. There is a close association between flowering plants and birds that do not subsist on fish; thus the absence of flowering plants in any spot would account for the absence of birds (other than those that subsist on fish) and of placental and marsupial mammals. The reason why no fossils of fishing birds have been found earlier than they have, may be because birds are rarely fossilised. I am inclined to think that the Cretaceous and Jurassic birds known to us subsisted on hard-scaled ganoid fish and that is why they had teeth; and that these birds died out with the decline of the ganoids. Does Dr. Davies seriously believe that the Eocene birds are descendants of toothed birds which, with one accord, shed their teeth at the end of the Cretaceous, and that Odontopteryx, missing its lost teeth, grew false ones?

When and how the Eocene birds were created I do not know, and this applies equally to the Cretaceous and Jurassic birds.

Dr. Davies takes exception (E. p. 192) to my assertions that Archaeopteryx and Archaeornis do not suggest the order of reptiles which gave birth to birds and that there is no agreement among evolutionists as to which group did. He rejects the Pterosaurs as possible ancestors and says that this limits the possible reptilian ancestors to three closely connected groups; Thecodontia, Saurischia and Ornithischia. He is of course, entitled to this view, but some do not agree with him: thus Nopcsa and Versluys would derive birds from the order Rhynchocephalia, while Furbringer's opinion is that birds cannot be derived from any particular type of reptile. Those who wish to find out what divergence of opinion exists as to the origin of birds are invited to read pp. 181-186 of Berg's "Nomogenesis."

My main point, however, is that Archaeopteryx is so good a bird that its discovery throws no light on the supposed evolutionary origin of birds. Dr. Davies complains (E. p. 191) that I omitted to mention "the thoroughly reptilian skull, the simple vertebrae devoid of saddle-shaped articulations and the non-pneumatic character of the bones" of Archaeopteryx.

As to the skull, I do not agree that it is thoroughly reptilian, I consider it typically avain. I believe this is the view of the majority. Of the writers I have consulted Heilmann is the only one who agrees with Dr. Davies, but the drawings of the skull in Heilmann's "History of Birds" show this to be bird-like. "The skull" writes Sir A. Smith Woodward (Z. vol. 2, p. 436) is shaped like that of a typical bird, its constituent

elements being fused together and the quadrate bone apparently free." As to the vertebrae, it is true that those having saddle-shaped articulations are found only in birds, but this is not necessarily an avian character; cormorants, darters, gulls and some parrots do not show it.

Pneumatic bones are not a prerogative of birds; they are present in some reptiles (Pterodactyls and Dinosaurs) and lacking in some birds. According to Dr. Crisp (*teste* Seeley "Dragons of the Air" p. 47) the swallow, martin, snipe, canary, wood-wren, willow-wren, whinchat, glossy starling, spotted flycatcher, and black-headed bunting have no air in their bones. It is open to a critic to assert that I have overrated the reptilian characters of Archaeopteryx, because (1) when I said the tail vertebrae were not fused, I omitted to mention that they are not completely fused in the ostrich (2) I did not mention that the long tail is not necessarily a reptilian character, the tail being very short in some Pterodactyls, (3) when stating that the metacarpels are not fused I did not mention that Mr. W. P. Pycraft says that they seem to have been fused but not so completely as to obliterate the tell-tale sutures ("History of Birds" p. 374), or that the metacarpels of the extinct Eocene, Gastornis, are free.

Archaeopteryx, although a good bird, was a peculiar one and seems to have had peculiar habits, but I do not deem it a link between birds and reptiles.

Dr. Davies is of opinion that there is a serious fallacy in my assertion that an animal is peculiarly liable to meet with accidents while acquiring the power of flight. He has drawn a graph (E. p. 196) purporting to show that the chances of fossilisation are greater for stable

than for evolving species. I submit that he has overlooked two important facts.

The first of these is that the birth rate of a species depends to some extent on the density of the population; the greater the density the lower the birth rate. Mr. G. L. McAtee adduces evidence of this ("Scientific Monthly" vol. xlii. pp. 444-456). Thus, he writes "In extended studies of the house wren, Hendeigh found that the number of broods per female per season tends to vary inversely with the total population. On a certain tract in Ohio where the number of nests was 7 the number of broods per female was 1.8; when the number of nests was 14 the broods fell to 1.2 per female."

The second fact not taken into consideration by Dr. Davies is that the greatest mortality is among immature animals, in the egg and nestling stage in the case of birds.

An enquiry made by me in India into the causes of mortality among birds, the results of which are summarised in "Indian Bird Life," demonstrates this. So far as I am aware, every one who has conducted a similar enquiry has obtained similar results. Mr. McAtee records figures (Quart. Rev. Biology. vol. xii., pp. 47-64) indicating that from 70 to 80 per cent. of the total mortality is among eggs and young birds. The chances of these immature victims becoming fossilised is almost *nil.*

Bearing these facts in mind, let us consider a population of land animals numbering four millions just before they are about to develop powers of flight. Let us assume that these lay twenty million eggs in the year and the population is stationary. This means twenty million deaths per annum, of which some 75 per cent., or

fifteen millions are those of eggs and immature birds. In order not to overestimate, reduce this figure to 60 per cent., or twelve million, and put the adult deaths at, not five millions but eight. Of these deaths some, as from bogging in mud or quicksands, from floods, hail, volcanic ash, etc., are likely to result in fossilisation. Let us put the percentage of such accidents at 0.5 of the whole i.e. 4000 out of eight million. Let us assume that only one of the 4000 victims of such accidents actually becomes fossilised. This figure is probably far too high. It does not matter for our purpose if it be decreased a millionfold. Now this four million population begins to try to fly and, on account of its incompletely developed wings, suffers many additional accidents, let us say a million of these, of which half are such as may result in fossilisation and that one in 4000 of these does so result; this would mean that the number of fossils laid down in the year would increase from 1 to 127. As such additional loss will be partly made good in the following year by increased fertility, nearly 127 fossils will be laid down in the following year; and for a long period the number of fossils laid down annually will exceed one. If further, some of the species attempting to become fliers are wiped out in the attempt, the number of fossils of incipient fliers will be considerably increased.

Dr. Davies makes no attempt to meet the difficulty raised by me as to how such a structure as a feather can have originated gradually, nor has he attempted to explain why the wing of the pterodactyl differs fundamentally from that of a bird, why the efforts of one kind of reptile to fly produced the bird's wing and those of another resulted in the wing of the

pterodactyl.

The presence of an ambiens muscle in some species and its absence in other species of the same family, while very embarrassing to the evolutionist who believes man's external ear-muscles to be useless vestiges of former well-developed muscles, is merely a point of interest to the creationist, because it raises the question whether the units of creation may not be smaller than the natural family or whether each family may not have been created in the form of several pairs of individuals, each having peculiarities. As I have stated, I have an open mind on the subject of the units of creation. This is a matter on which it seems useless to speculate in the present state of knowledge. I hope that the day is not far distant when biologists, having freed themselves of the dogma of transformism, will endeavour to determine the extent to which differentiation is possible.

Geneticists should be able to elucidate this by subjecting the animals and plants on which they experiment to conditions of a kind likely to occur in nature, in preference to bombarding them with X rays.

CHAPTER XI.

Reptiles and Mammals

In a chapter entitled "Origin and Evolution of Mammals" Dr. Davies discusses the difficulties confronting those who believe that in the past a reptile became transformed into a mammal.

The living reptiles are a small remnant of a great group more diversified than mammals are to-day. The differences between a mouse, an elephant, a bat and a whale are not so great as those between some extinct reptiles.

Habits and form are closely connected. As animals of different orders may have similar habits, they may resemble one another in form. Dr. Davies (E. p. 153) cites some instances of this. Thus, similarity of form does not necessarily denote close blood relationship. Powerful jaws, and teeth for cutting, tearing and grinding were just as necessary to the reptiles that preyed on large quarry as they are to carnivorous mammals. Therefore, whether or not mammals be derived from reptiles, we should expect to find the teeth of some of the extinct reptiles like those of carnivorous mammals, especially as the former exhibited far greater variety of teeth than do mammals. These expectations are realised. A large extinct group of reptiles is known as mammal-like reptiles or Therapsids, because of their skeletal resemblances to mammals. The fossil skull,

named Tritylodon longaevus, found in a South African Triassic deposit, is deemed by some to be that of a reptile, by others to be that of a mammal, belonging to the extinct Allotheria.

The problem to be solved is; are these resemblances due to close kinship or to similarity of habits? The solution is rendered very difficult because we have no direct knowledge of the soft parts of these extinct reptiles, and although there are a number of skeletal differences between all existing reptiles and mammals, there is no physiological reason why a reptile should not have a skeleton very like that of a mammal. It is in the soft parts that the fundamental differences between mammals and reptiles are seen. "A reptile" writes Vialleton (O. p. 118) "does not differ from mammals only because it has but one occipital condyle, and other skeletal features different from those of mammals, but because its integument is altogether different from that of the latter. Its epidermis lacks the granular layer, and its surface, strongly corneous, undergoes but one desquamation a year, coinciding with a general crisis of its nutrition, which does not occur in mammals. Its dermis lacks papillae. It has no hairs. Its bony tissue owes little to enchondrial ossification; its epiphyses are very different from those of mammals, as is the distribution of its fat; its excretion is by means of uric acid and not urea, etc., etc." Some other differences in the soft parts are set forth in my former volume (D. pp. 74-6).

When endeavouring to decide whether the skeletal resemblances of the mammal-like reptiles to mammals are due to similarity of habit or to blood relationship it is necessary to bear in mind certain facts.

1. So great is the variety displayed by vertebrates that inevitably some members of each class display resemblances to members of other classes; for example the mammal known as the duck-billed platypus exhibits some avian and some reptilian characters.

2. The extinct flying reptiles, known as Pterosaurs or Pterodactyls, display many bird-like features. H. G. Seeley writes ("Dragons of the Air" p. 222) "The avian characters of Pterodactyls are predominant parts of their organisation, for the conditions of the brain and lungs shown by the moulds of the brain-case and the thin hollow bones with conspicuous pneumatic foramina, give evidence of a community of vital structures with Birds, which is supported by characters of the skeleton.....A community of structures is found to extend among the bones of such distinctive parts of the skeleton as the sternum, shoulder-girdle, bones of the fore-arm and fore-leg; for in all these regions the Pterodactyl bones are practically indistinguishable from those of Birds. This is the more remarkable because other parts of the skeleton, such as the humerus and pelvis, show a partial resemblance, while the parts which are least avian, like the neck bones, have no tendency to vary the number of vertebrae in the way which is common among Birds."

In addition to the above, the skull of the Pterodactyl exhibits a number of avian features.

Thus, in the case of the Pterodactyls and the Birds the same question arises as in that of the Therapsid Reptiles and the Mammals: are these resemblances due to near blood relationship or to like habits?

At one time it was thought by many that birds are derived from Pterodactyls; to-day few, if any, hold this view, for several reasons, the chief of which is that the

bird's wing is constructed on a plan fundamentally different from that of a Pterodactyl. Evolutionists generally believe that the bird-like features of the Pterodactyl are either those "common to most archaeosaurians or parallel developments due to similarity of life." (E. p. 193).

On the other hand the resemblances of Therapsids to mammals, although certainly not greater than those of Pterodactyls to birds, are deemed to be due to kinship.

3. This group of reptiles is not the only one exhibiting mammal-like features. Prof. L. M. Berg writes (N. p. 197) : "In the Permian and Triassic Dicynodon, of the order Anomodontia, the structure of the zygomatic arch, scapula and pelvis is like that of mammals. If we admit that Mammalia were derived from Theriodontia, by what.....may be explained the similarity of Dicynodon to mammals? In Dinocephalia...in the region of the ear are displayed remarkable resemblances to what is observed in mammals. Lastly in the Pareiosauri.....we find a development in the direction of mammals, to which the presence of an acromion and the fusion of the pelvic bones testify. Thus, even if we admit that mammals have been derived from one group of Theromorpha, it follows that the resemblance of the former to other groups of the latter is due to parallel development."

In Berg's opinion the mammals cannot be derived from the Theriodontia. He, with Furbringer, believes that their mutual similarities are due to parallel development, i.e. to similarity of habits or to coincidence. In my opinion the above three considerations are of less importance than the physiological difficulties involved in the supposed transformation of a reptile into a mammal.

Transformists are inclined to gloss over these difficulties. Thus, Dr. Davies seems to think that the fact that we can imagine how such a transformation might have been effected is evidence that it was effected, for example, as it is theoretically possible to derive the mammalian type of hip-girdle from that of a reptile, the transformation presents no difficulty.

As to the origin of warm-bloodedness, Dr. Davies' supposition that some extinct reptiles may have had slightly warm blood and not very efficient mechanism for regulating this does not explain how or why a reptile acquired the extravagant habit of warming its blood and even an inefficient means of keeping up this warmth.

As to the origin of the diaphragm, Dr. Davies writes (E. p. 206) : "It is doubtful if we shall ever learn how the diaphragm arose, with the gradual substitution of a thoracic for a buccal breathing-mechanism; but this does not mean that it could not have arisen by natural evolution." It certainly does not necessarily mean that it did arise gradually. What can have caused any animal to change its mode of respiration?

As to the complicated Organ of Corti, Dr. Davies writes (E. p. 209) : "Increasing elaboration of sense organs is a natural feature of evolution towards a higher type of life and an Organ of Corti would have been useless in the absence of the refinement of transmission due to the substitution of a chain of small ossicles for the rod-like columella auris." Does Dr. Davies really believe that he has thus explained the origin of the Organ of Corti?

He makes no attempt to meet the difficulties regarding the musculature of the intestine, the lack of nucleus

in the red-blood corpuscles, the non-striped ciliary muscle and the different system of blood supply to the iris of the eye. He candidly admits that the supposed scrapping of the original articulation of the lower jaw with the skull and the substitution of a new hinge is "more plausibly explained by the intervention of creative power than by gradual evolution." Not so Professor R. Broom who writes ("The Mammal-like Reptiles of South Africa" p. 315) : "The snapping jaw had to be converted into a masticatory jaw, and, as the quadrate became more or less fixed to the squamosal, it kept with it the articular and the other little bones of the jaw, and the dentary became practically free and formed a new hinge with the squamosal. The small bones no longer moving with the jaw gradually became modified as part of the auditory apparatus."

For my part I regard as fantastic the notion that all the changes necessary to convert a reptile into a mammal took place gradually. When a geneticist shall have effected these changes in some reptile it will be time enough to believe that they happened gradually in nature. I submit that in the present state of knowledge the scientific attitude is that, if in the past a reptile became converted into a mammal, the conversion was effected miraculously and suddenly.

Dr. Davies takes exception to my assertion (D. p. 134) that the evolutionist explains the late appearance of the monotremes in the rocks known to us by declaring that they evolved from some unknown ancestor in some unknown part of the world, and, after they had fully evolved, migrated to Australia. His view is that some mammal-like reptile migrated to Australia before the Dinosaurs obtained a foothold there, and, after arrival,

changed into a monotreme, and that shortly after, Australia was cut off by the sea from the rest of the world. But apparently Dinosaurs did reach Australia; Messrs. Gregory and Barrett write ("General Stratigraphy," p. 162) : "In Victoria, Jurassic shales and sandstones include volcanic debris, and a few coal seams with Taeniopteris and the claw of a Dinosaur allied to Megalosaurus." Moreover, if Dr. Davies' view be correct, it is reasonable to suppose that fossils of monotremes would have been discovered in some of the Secondary and Tertiary land deposits of Australia, such as the Jurassic Clarence Series of New South Wales, the Ipswich Series of Queensland and the Leigh's Creek formation in South Australia; the Cretaceous Barremian Coal Beds of Queensland, the Eocene deposits in New South Wales and Victoria, and the Oligocene Balcombrian Marls of Victoria and the Queensland rocks mentioned by Dr. Davies. As no fossils of monotremes or their ancestors have been found in any of the above rocks, it is rather rash to assert that these lived in Australia in the long periods during which the above rocks were laid down.

It is open to doubt whether any mammalian fossil of earlier date than Pliocene has been found in Australia. Dr. Davies does not accept this statement, for he asserts that the marine bed in which the marsupial Wynwardia bassania was found is generally accepted as Miocene; but Professor W. K. Gregory states (B., vol. 14, p. 979) that this deposit is now known to be Pliocene. Baldwin Smith, who first described Wynwardia, deemed the deposit in which it occurs to be of Eocene date and the creature itself to be a composite type exhibiting kangaroo, dasyure and wombat features, and so regarded it

as an ancestor of these last three creatures; but gradually the date of this deposit has been advanced until, as we have just noticed, Professor Gregory says it is now known to be Pliocene. If this be so, or even if, as Dr. Davies thinks, it be Miocene, Wynwardia is too recent to be an ancestor of the kangaroos, dasyures and wombats. It would appear that this also applies to the extinct Australian genera Diprodonton and Nothotherium, which, says Dr. Davies, (E. p. 212) are not referable to any existing family, being 'annectant types' (or links) between kangaroos, wombats and phalangers, because all these living types have been found fossil in deposits as old as those that contain Diprodonton and Nothotherium.

The evolutionary origin of the Marsupials has become more difficult to account for than formerly, because, as Dr. Davies mentions (E. p. 219), many zoologists now believe that "the marsupial condition must have been reached by degeneration from a primitive placental stage." New discoveries tend to increase the difficulties of the evolution theory and to lead to its being frequently modified. Dr. Davies writes (E. p. 219) that whether the marsupials "originated in the Northern or the Southern Hemisphere is a matter of guesswork. My own guess would be that they originated in the Southern Hemisphere, either in South Australia or South America or some land connecting the two."

He may be right; who knows?

CHAPTER XII.

Man

Dr. Davies asserts (E. p. 223): "If Evolution be accepted as true of living things in general, Man cannot be excluded. He bears too many stigmata of his relationship to other animals."

As an immense mental or psychic chasm separates man from the rest of creation, Dr. Davies might have been expected to show how the chasm has been bridged, how man's intellectual powers and his power of speech were acquired by some animal. This expectation is not realised. Dr. Davies scarcely notices the attributes that distinguish man chiefly from all the rest of creation. He devotes a page to "the psychic side of life" in the course of which he writes (E. p. 249): "That Consciousness, Memory and Personality have been gradually developed, step by step with the evolution of bodily structure and function seems unquestionable, yet we can frame no theory of how the two sides of this double process are connected, nor how their association first began."

That this immense difficulty should thus be airily brushed aside, is typical of the way in which adherents of the evolution theory slur over its difficulties.

There is not a particle of evidence that man's mental powers have increased since his appearance on the earth. Dr. Davies cannot comprehend how man invented the wheel, yet ancient man seems to have done this.

A. R. Wallace fully appreciated the immensity of the gulf between man and the animals. He wrote ("Darwinism" p. 475) : man's mental faculties "could not possibly have been developed by means of the same laws which have determined the progressive development of the world in general, and also of man's physical organism."

As regards the remarks made by Dr. Davies concerning man's physical organism, I have shown that the "tail" in the human embryo and the innervation of the eye present no difficulties to the creationist. (see p. 35).

As the crudities of the notion that man's body gradually evolved from that of some animal have been exposed in my volume "Man : A Special Creation" all that I need here say on this matter is that it is one about which much nonsense has been written. The following gem is culled from page 75 of Dr. John Murphy's "Primitive Man," a book published by the Oxford University Press : "The erect position of the body, which means so much for the growth of the brain and thus of the mind, is only attained in evolution slowly, and so to say with difficulty. The stages from the lemur-like quadruped to Pithecanthropus erectus and the European Apollo or athlete, might easily be traced. The gorilla, for example, supports the erect posture by resting on the knuckles of the hand against the ground. The orang-utan may grasp a branch overhead as he stands, or walk with his arms balancing above his head. When the upright position was new to the precursor of man, the necessity for frequent rests would be greatly felt." Is not this a delightful picture of the gallant precursor of man laboriously practising at erecting himself on his hind legs, and each time he drops on all fours,

thoroughly exhausted, consoling himself by the thought "Cheer up Culley. You'll soon become Homo sapiens!" A little reflection should to suffice to convince anyone of the great improbability of such valuable endowments as an opposable great toe and a hairy covering becoming lost, and that the notion that some quadruped became gradually converted into a biped is fantastic. If man be derived from a quadruped, having a coat of long hair and an opposable great toe (of which there is not a shred of evidence), the transformation must have been effected *per saltum* miraculously.

In the matter of hairs it should be noted that, apart from the head, there are probably more hairs on man's body than on that of an anthropoid ape, but over most of the human body the hairs are so fine and short as to be of no use as a protection against cold or skin abrasions. They, however, are essential to man's well being; each of these fine hairs is provided with a muscle capable of moving it. The function of these hairs is to regulate the flow of the oily fluid secreted by the sebaceous glands that open into the follicle or pit in which each hair is embedded.

Doubtless the fine coat of hair known as lanugo which covers the human foetus from the seventh month has similar functions in connection with the secretion of the large sebaceous glands of the foetus, forming the vernix caseosa with which the foetus is covered until birth.

All or nearly all the lanugo is shed before birth, to be replaced after by the fine hairs or vellum that occur on most parts of the body. Those who deem the lanugo of the human foetus to be a relic of the adult hairy coat of man's ancestors, have to believe that, after the original protective coat was shed, the incipient human

proceeded to grow the present non-protective covering of very fine hairs!

As the chimpanzee foetus is covered with a coat of lanugo which is shed at birth, those who regard lanugo as a useless relic must, if logical, believe that a comparatively recent ancestor of the chimpanzee lost its hairy covering, and then, missing this badly, proceeded to grow a new thick coat!

So much for man's stigmata of his supposed hairy ancestors.

Dr. Davies is of opinion that if all the fossils of men and anthropoid apes found since the publication of Darwin's "Descent of Man" had been unearthed simultaneously the cumulative effect of the evidence would have been overwhelming. I would substitute "bewildering" for the last word. According to Sir Arthur Keith, Dubois withheld for more than twenty years the fact that he had found fossils of two-large-brained men before he discovered Pithecanthropus, because ("The Antiquity of Man" p. 441): "There is no doubt that if, on his return in 1894, he had placed before the anthropologists of the time the ape-like skull from Trinil side by side with the great-brained skulls from Wadjak, both fossilised, both from the same region of Java, he would have given them a meal beyond their powers of digestion."

In my opinion the known fossils throw no light whatever on the supposed animal ancestry of man. They do show that men and anthropoid apes were formerly more diversified than they now are, and that, then as now, small- and big-brained men existed contemporaneously.

That the modern type of man is very ancient is shown

by the Castenedolo, the Olmo, and the Calaveras fossil skulls, the last of which Sir Arthur Keith calls (op. cit. p. 471) "the bogey which haunts the student of early man," also by the Oldoway and Clichy skeletons, and the Abbeville, the Foxhall, the Kanam and Kanjera fossil jaws (vide F. pp. 150-161). These discoveries of fossils of men of modern type in deposits certainly at least as old as, probably older than, those containing the remains of Sinanthropus, Pithecanthropus * and Australopithecus exclude these latter from man's ancestry.

This fact, however, does not prevent Dr. G. H. R. von Koenigswald from writing of "our ancestor Pithecanthropus" (Illustrated London News, December 11th 1937). Nor do the facts that the Castenedolo skull, believed to be that of a woman, has a brain capacity of about 1340 cc. (about that of an average modern European woman), while that of a modern Australian bushman is about 1250 cc. and the largest living ape 620 cc. prevent Koenigswald writing "We now see that there is not much difference in brain capacity between the earliest fossil men and the largest living anthropoid ape."

Despite a search extending over the greater part of a century—a search which has brought to light a considerable number of fossils of men and primates, it is not possible to point to any fossil and say of it: the species represented by that fossil, while not human, is

* Dr. Davies writes (E. p. 239): "The present balance of opinion is in favour of an early Pleistocene age for the Trinil bone-bed in which the fragments of Pithecanthropus were found." I doubt this. Branca, Martin, Volz and Elbert went into this question and insisted on a Middle Pleistocene date. Koenigswald writes, as recently as 11th December, 1937 (Illus. Lon News), of Pithecanthropus: "his geological age is, according to the mammalian fauna of the Trinal layers not older than Middle Pleistocene," but he adds that the recently found Modjokerto fossil indicates that man, "probably Pithecanthropus remains, already existed in Java in the Lower Pleistocene Period." But even this is later than the human Kanam and Castenedolo fossils.

that of an ancestor of man."

I have no hesitation in asserting that the majority of the fossils of man and the higher primates discovered during the last fifty years are not in accord with the expectations of evolutionists.

The idea that man is derived from an ape has been discarded by some modern biologists. Thus, Professor Max Westenhofer writes ("Research and Progress," vol. 3 (1937) p. 92): "I am more and more convinced that the Darwin-Haeckel theory of the ascent of man from the ape cannot be supported any longer. In opposition to this theory I was able to show that man... can be traced back in a direct line to the general basic type of mammal, but not to any particular type of animal and especially not to the ape . . . . It was useless to search in prehistoric times for any missing link between man and the ape, for these links neither exist in the history of man nor in that of animals and plants, as my own observations have proved and has been shown by numerous modern and older investigations of the palaeontologists. All the larger groups of animals, e.g. fishes, amphibians, reptiles, mammals seem to have appeared suddenly on the earth, spreading themselves, so to speak, in an explosive manner in their various shapes and forms. Nowhere is one able to observe or prove the transition of one species into another, variations being possible only within the species themselves. Thus, man, too, has his own line which can be traced back to the type of the original mammal." Westenhofer, goes so far as to say that his view approaches "the old idea of Linnaeus, namely that there exist as many species as the Infinite Being has created."

Westenhofer, however, is not content with confining

himself to observed facts. He traces man back not only to an hypothetical general type of mammal, but to earlier ancestors which he says "are most probably to be found in the type (not in any special known form) of the tailed amphibians (Urodeles) and Chondrichthyes (Selacians.)."

Somewhat similar views are held by Sergi, Berg, Belogolovy, Haack, Kleinschmidt and others.

This theory, which is one of special creation coupled with evolution, is an attempt to square the stability of species, as demonstrated by breeding experiments, with the fact that the farther we go back in time the less do the flora and fauna resemble those of to-day.

Consider for a moment the implications of the theory that each of the existing species was separately created in the form of a one-celled-organism, which, in course of time, assumed the present form of the species.

There exist to-day some 15,000 species of mammal, 30,000 of birds, 5,000 of reptiles, 3,000 of amphibians, 20,000 of fishes and some 400,000 invertebrate Metazoa. If all these be descended from different one-celled ancestors, it involves the independent conversion of some 350,000 Protozoans into Metazoans, some 80,000 invertebrates into fish, some 60,000 fish into amphibians, some 50,000 amphibians into reptiles, and 20,000 of these into birds and 15,000 into mammals.

Some of these one-celled creatures thus created must have remained in the one-celled condition and constituted the various orders of the Protozoa.

The majority must have become many-celled organisms, and of these the greater number have developed only to some invertebrate stage, while of those that became vertebrates some are still in the amphibian

stage, others in the reptile stage.

If this type of evolution had occured and were still in progress, we ought to see among the vast assembly of living species some invertebrates well on the way to become fish, some fish almost amphibians, and some amphibians on the verge of becoming reptiles or birds or mammals. We see nothing of the kind. All the great groups are to-day sharply defined, and there is no species which cannot readily be fitted into one or other of the great groups.

As knowledge increases biologists are being driven, slowly but surely, to a belief in the special creation, if not of every species, at least of all the types of animals and plants. Either these were all created at the beginning of Cambrian time, or successively at various periods. At present our knowledge is insufficient to enable us to decide this matter, but the geological record, so far as it is known, seems to point to the latter alternative; but in this connection we must bear in mind that, as our knowledge of the fossils increases, it becomes necessary to push farther and farther back the date of the appearance in the rocks known to us of the various higher types of animals and plants.

CHAPTER XIII.

Difficulties Presented by Some Instincts.

Many habits of animals are such that it seems impossible for them to have been acquired gradually. One of these is the habit of the male of the fish Arius of incubating the eggs in his mouth—a habit necessitating his going without food for several days. The aft-top-sail fish (Felichthys felis) and the toad (Rhinoderma darwini) have similar habits, as have the females of African cichlid fishes.

The nesting habits of many birds cannot have developed gradually. Let us notice those of the Indian tailor-bird, the oriole and the weaver-bird. The hen Indian tailor-bird (Orthotomus sutorius) makes a receptacle for her eggs out of one or more leaves while still growing on a bush. The procedure differs as she utilises one large leaf or two or more small ones to form the casing of her nest. When a single leaf is used the hen begins operations by piercing with her bill a series of holes along each margin of the leaf. She then procures strand after strand of cobweb. One end of each strand is wound by her round the part of the leaf between the margin and one of the holes she has punctured. Having thus made this end fast she attaches the other end of the strand in the same way to the opposite edge of the leaf, thereby drawing its edges together. She then connects in the same manner each of the other punc-

tures with the corresponding one near the other edge of the leaf. Thus a cone-shaped receptacle is formed. This may be more or less horizontal at first, but gradually assumes a vertical or hanging position as it becomes weighed down by the weight of the soft cotton-like material of which she makes the actual nest. The bird keeps this material in position by puncturing with her bill a number of holes in the leaf, through which she pushes some cotton which is retained in this position by the silicious material in the leaf. Although cobweb bears considerable strain, it could not keep together the edges of the leaf when this held the mother bird and three or four nestlings. The hen therefore connects some of the punctures on opposite sides of the leaf by short strands of cotton, each end of each strand being pushed through a puncture. The fluffy knob of cotton protruding through the puncture, which is kept *in situ* by the silicon in the leaf, has been mistaken by some observers for a knot, but the bird does not make a knot. I have seen dozens of these wonderful nests, but never one that did not bear the weight of the mother bird and her brood. It seems impossible that the habit of making a nest in this way can have evolved gradually.

The Oriole (Oriolus kundoo) constructs a hammock slung from the prongs of a forked branch. The hen either shaves off strips of pliable bark up to two feet in length, or makes use of other suitable material, such as hemp fibre, or even strips of cloth. Having secured a suitable strip, the hen winds one end round one limb of the selected forked branch, carries the free end to the other limb, winds the strip around it and, returning to the first limb, winds the end of the strip

round this. Several fibres dealt with in the above manner serve to support the nest which is a slender cup made of fine fibres.

The nest of the weaver bird (Ploceus baya) is a superbly plaited flask suspended from a branch of a tree. The material used varies with the locality. In Bombay it usually consists of thin strips of cocoa-nut leaves; elsewhere it is commonly composed of strips of giant grasses. Having selected, let us say, a blade of elephant grass, the bird makes a notch with its bill in the blade near its base.

The weaver then grips with its bill the edge of the grass at a point just above the notch, and, with a jerk of the head tears a thin strip from the leaf.

While retaining this in the bill, it tears off a second, and a third, and occasionally a fourth strand; it then flies off with its load of building material. Should any of the strands still remain attached to the tip of the grass stem the momentum of the flying bird usually suffices to complete the severance. If it does not the flier is pulled back and swings in the air suspended by the still fastened strips. In that case the bird again flies off and perseveres until it succeeds. The fibres first collected are wound round and round the branch from which the nest will hang. These remain firmly fixed owing to the silicious matter in the leaf. The fibres next collected are plaited into those attached to the branch and in a short time the birds have plaited a rope some four inches long. This is then expanded by the addition of further material into a bell-shaped structure, which will form the roof of the nest. The next step is to plait a loop across the base of the bell, which then has the appearance of an inverted basket having

a handle. Up to this point both cock and hen do the same kind of work. After the loop is completed the hen brings in no more material but remains perched on the loop while the cock does the fetching, and both plait the material he has brought, the hen working from the inside and the cock from the outside of the nest. The next step is to close up one side of the loop to form a receptacle for the eggs; the other side is left open, but is prolonged downwards to form a tubular passage about six inches long.

Thus, the entrance to the nest is from below and the nest has the shape of a retort. The habit of constructing a nest of this description cannot have been developed gradually. This applies to many other kinds of nests, that of the English house-martin, for example. This is a cup composed of mud pellets attached to a wall under the eaves of a house.

A great many examples of habits that cannot have been acquired gradually or piecemeal are afforded by spiders and insects. The books of Henri Fabre are full of them. Even though Fabre on occasions may have exaggerated the accuracy with which the solitary wasps find the nerve ganglia of their victims in order to paralyse these, it is impossible to believe that such habits developed gradually in many species, even in a form not so perfect as they now present.

Facts such as these led Fabre to regard the theory of organic evolution as a solemn hoax.

PART II—PALAEONTOLOGICAL.

CHAPTER XIV.

The Dating of Geological Deposits

At the time I wrote "Difficulties of the Evolution Theory" I had paid little attention to geology, apart from its palaeontological aspect. I accepted, with some hesitation, the prevalent views of the ages of the various fossiliferous rocks, based on the rates at which the elements Uranium and Thorium disintegrate. Having since gone into this matter I am forced to the conclusion that this method of computing the age of any deposit is unsound.

It has been as eagerly accepted by biologists and palaeontolgists as Darwin's theory was. Those who accepted these theories seem to have acted on the adage "Don't look a gift horse in the mouth." The stupendous transformations of plants and animals postulated by the evolution theory demand an immense stretch of time, which, until recently, physicists were unwilling to allow. The radio-active test of the age of the earth alters all this. Evolutionists, therefore, could hardly be expected to examine this test critically.

Since I have discarded the theory of evolution it has become a matter of indifference to me whether this method of computing the age of rocks be sound or unsound; accordingly I have been able to examine it critically. The weakness of the method is that it involves a number of assumptions which may be incorrect:

1. That the rate of disintegration of Uranium and Thorium has always been the same. There are various reasons for thinking that formerly this was much more rapid than it now is. Mme. Joliot gives some of these and Dr. Joly others ("The Surface History of the Earth" (1925) p. 148 *et. seq.*). If this be the case, the current estimates of the ages of the rocks are too high.

2. That there formerly existed no isotopes of Uranium and Thorium which disintegrated more rapidly than the forms of these elements known to us. Dr. Joly is of opinion that in the case of Uranium there is definite evidence of the former existence of such an isotope.

3. That Uranium and Thorium themselves, unlike Radium, are not stages in the disintegration of elements of higher atomic number, which broke up so rapidly that they have ceased to exist on earth. There is no evidence that this is the case, but we cannot be sure it is not. Indeed Professor Fermi and others have been able to produce in the laboratory elements of higher atomic number than Uranium, which broke up very rapidly.

4. That of the 16 known isotopes of Lead only two, viz. Uranium-lead and Thorium-lead are the products of radio-disintegration. For all we know some or all the other 14 isotopes may the end-products of elements which broke up so rapidly that they no longer exist on the earth. This is not a wild guess. The late Lord Rutherford, when dealing with the unstable elements formed under the observations of M. and Mme. Curie-Joliot, wrote in the paper read before the Indian Science Congress on January 3rd, 1938 : "No doubt such transient radio-active elements are still produced by transmutation in the furnace of our sun, where the thermal motions of the atoms must be very great. These radio-

active elements would rapidly disappear as soon as the earth cooled down after separation from the sun. On this view Uranium and Thorium are to be regarded as practically the sole survivors on our earth of a large group of radio-active elements owing to the fact that their time of transformation is long compared with the age of our planet." ("Nature" (1938) p. 60.)

Professor Joly writes (*op. cit.* p. 145) : "The study of haloes.....affords evidence.....of the former existence of yet other radio-active elements."

That some, at least, of the above objections to the radio-active method of calculating the age of rocks are valid is shown by the following facts :

(a) The very discordant results obtained from ores in the same rocks.

Here are some examples. Dr. Joly records (*op. cit.* p. 152) : " A selected specimen of thorite from Ceylon afforded an age of 150 million years, whereas a uranium-lead ratio gave these rocks 512 millions of years."

Mr. D. J. Whitney (Trans. Victoria Inst. 1933, p. 34) stated that examinations of certain Texas ores would make these ores, all from the same deposit, vary from 1,671 to 11,470 million years old.

Professor A. Holmes states (Jour. Amer. Sci. (1934) p. 143) the lead ratios from several specimens of uraninite from the same geological formation in Gordonia, S. Africa, gave lead ratios varying from 0.118 to 0.172, a variation of nearly fifty per cent. He deems 0.131 to be the correct ratio, but he admits that the ore which gave the highest ratio was specially selected on account of its fresh appearance and that neither the chemical nor the physical properties of this specimen were such as to suggest that its lead ratio was too high by over

fifty per cent.

Obviously there is something radically wrong with a method of computing age which gives very discrepant results.

(b) The results of the radio-active method of dating the age of rocks are quite incompatible with those based on the sodium content of the oceans.

The rivers are unceasingly pouring quantities of sodium and other salts into the sea, in dilute solution. The amount of water in the oceans is kept constant by evaporation, but the water thus evaporated is free from salts.

As there is no known means by which the sea is able to rid itself of more than a small fraction of the sodium carried to it, it is evident that the ocean must be growing salter. We can calculate with fair accuracy the amount of sodium carried to the sea by rivers every year and the total sodium content of the oceans. These figures are respectively 156 million and 1,260 million million tons. If we divide the latter figure by the former we get 81 million. This would represent the number of years it would take for the sea to have acquired all the sodium it contains, if the whole of this be derived from the rivers, assuming that the rivers have all along brought in sodium at the present rate.

Making allowances for the possibility of the rate having been slower formerly and for the sea getting rid of some of its sodium as spray and in other ways, Professor Sollas, assuming that the sea originally contained no sodium, arrived at the extreme limits of the existence of the oceans as from 80 to 175 million years (Presid. Address, Geolog. Soc. 1909). Nevertheless geologists today assert that the sea has been in existence

considerably more than 600 million years! It is true that Dr. A. Holmes has attempted to show ("The Age of the Earth," p. 39) that if we exclude the sodium carried to the sea otherwise than in solution we arrive at the figure 330 million years as the age of the ocean. But even this figure is far lower than that given by the radio-active method, and is based on the assumption that the sea was originally entirely devoid of sodium. That this last assumption is incorrect and that the sea from its inception contained a large quantity of sodium is certain because the proportions of the various kinds of salts in river and in sea water are very different. If the sea be nothing but concentrated river water, the proportions of the various salts it contains should be the same, but they are not, as the following figures given by Julius Roth show:

Proportions of the total Salts in any given volume of Water

	Carbonates	Sulphates	Chlorides
River water ...	80%	13%	7%
Sea water ...	0.2%	10%	89%

As the sea originally contained a large quantity of sodium and other salts, its age must be very much less than that arrived at by dividing its total sodium content by the amount of sodium carried to it annually by the rivers.

Let it be clearly understood that the above method does not give the age of the oceans, but it does fix a maximum, which is very much less than the figure given by the radio-active method of calculation.

Thus, until we discover that the ocean has some means unknown to us at present of ridding itself of vast quan-

tities of sodium, it is most unscientific to assert that the oceans are much older than the maximum allowed by calculations based on sodium content, and to date rocks by adopting the radio-active method. Yet, this is precisely what both popular and scientific books do. Thus, Dr. Davies, following the bad example set by the majority of scientists, speaks (E. p. 191) of fifty or sixty million years elapsing between the date of the fossilisation of Archaeopteryx and of Hesperornis.

As Sir Ambrose Fleming insists emphatically (Trans. Vict. Inst. 1937)' there is at present no known reliable means of discovering the age of any ancient rock.

CHAPTER XV.

The Absence of Fossils in Pre-Cambrian Rocks

A most important piece of evidence opposed to the doctrine of evolution in all its forms, not cited in "Difficulties of the Evolution Theory" is the absence, or, as some believe, the extreme paucity, of fossils in Pre-Cambrian rocks.

At the beginning of the Cambrian period a great marine fauna appears abruptly; this includes, in addition to a recently reported vertebrate (a supposed fish scale),* fossils of members of all the other great groups, or phyla, of the animal kingdom, those of each group representing most of its classes and orders.

The supposed evolution from one-celled ancestors of all these complicated Cambrian animals: radiolarians, foraminifers, sponges, corals, jelly-fish, graptolites, star-fish, sea-cucumbers, trilobites, phyllopods and molluscs must have occupied a period at least as long as the Primary, Secondary and Tertiary periods added together. If, therefore, such evolution took place, the rocks underlying those laid down in the Cambrian Period should be richly fossiliferous. This is not the case; not one of these rocks has yielded what is undoubtedly a fossil.

In the words of Dr. Percy Raymond, the sudden appearance of this Cambrian fauna in the rocks presents

* Shull, "Evolution" (1936) p. 48.

a constant challenge to the evolutionist.

For over seventy years the earth has been ransacked in the hope of finding Pre-Cambrian fossils. Every discovery bearing the remotest resemblance to an organism or an organic product has been carefully preserved and minutely examined. Some enthusiasts have found what they deem to be Pre-Cambrian fossils, but nearly all these have been almost unanimously rejected.

In what follows I have drawn largely on the Presidential Address to the Palaeontological Society of America delivered by Dr. Percy Raymond in 1935.

The first to be discovered of these supposed Pre-Cambrian fossils was named Eozoon canadense by Dawson, who regarded it as a giant foraminifer. For years this was generally accepted as a fossil, by Darwin among others.

This view is no longer tenable. Dr. Raymond writes (Bul. Geo. Soc. Amer. Vol. 46, p.378) : "It is obvious that Eozoon is the product of two periods of alteration of the original sediment and can by no possibility represent an organic structure. There seems not the slightest chance that it can be organic."

Nor is this all, blocks of limestone enveloped in molten lava at Monte Somma, Vesuvius, have, by the absorption of silicates, developed into typical Eozoon! But, so great is the desire to discover Pre-Cambrian fossils that recently attempts have been made to revive Eozoon as a plant fossil.

Needless to state Sir Albert Seward in his discussion of supposed Pre-Cambrian fossils in his " Plant Life through the Ages " does not mention Eozoon.

In 1906 Walcott described a number of genera and species of what he believed to be the products of cal-

careous (blue-green) algae from the Pre-Cambrian of North America. To these he has given such names as Atikokania, Newlandia, Collenia, etc. The structure of these, however, is unlike the product of any known alga, and, as Raymond remarks, while it is always possible in the case of Ordovician and more recent deposits containing secretions of algae, to identify at least the genus of the alga, this is not the case with Walcott's supposed fossils. In Raymond's view, the most that can be said for these is, "if organic, it is more likely that they are calcareous algae than anything else." Holtedahl extinguishes even this ray of hope by demonstrating that precisely similar concretions have been found in situations that preclude their being made by organisms. Further Liesegang has shown that such structures can be made artificially in the laboratory. Moreover, Adam Sedgwick pointed out that there is no end to the forms assumed by dolomite structures.

These remarks seem to dispose of the branched stem-like forms found by Dr. Metzger in the Pre-Cambrian dolomites of Finland, which he calls Carelozoon jatulicum. This supposed fossil is not mentioned by Dr. Raymond, possibly for the above reasons, and also because the deposit in which it occurs underlies unfossiliferous sandstones. Like some of the other supposed Pre-Cambrian fossils, this occurs in rocks very much earlier than the Cambrian.

Walcott in 1915 and Gruner more recently have described what they believed to be the remains of bacteria in the Pre-Cambrian of North America. Walcott, as Raymond points out, "Makes no argument in favour of the identification and leaves it to be accepted on faith that an organism without hard parts and less than .001

millimetre in diameter could be preserved in identifiable condition from Pre-Cambrian time to the present." This criticism applies with greater force to Gruner's finds, which are from much older rocks; Sir Albert Seward writes: these finds " though worth recording are by no means convincing."

The structure found by Sederholm in Finland, which he names Corycium enigmaticum and deems to be a product of some unknown plant, is not noticed by Raymond. Seward describes it as a problematical body and thinks it is inorganic. The last of the supposed Pre-Cambrian plant fossils consists of some carbonaceous material found near Prague by Dr. Krausel. He thinks it is part of a conifer-like plant: it has been named Archaeoxylon Krasseri. Seward writes of it: the weak point is that its Pre-Cambrian age has not been proved and its structure is too imperfect to admit of any satisfactory determination.

Turning now to the supposed animal fossils, that named Orthoceras on the supposition that it is a mollusc, found in the Pre-Cambrian Waterberg Sandstones of South Africa, has proved to be an inorganic concretion. Gregory and Barrett assert (" General Stratigraphy " p. 23) " The Torridon Sandstone of Loch Broom contains grains of phosphate with well preserved organic canals." No name is given to this supposed fossil, nor are the reasons stated for the supposition that the canals are organic. This may be why Raymond does not notice the supposed fossil.

Cayeux believes he has discovered in the Pre-Cambrian rocks of Brittany a collection of fossils composed of 45 species of radiolarians, 6 of foraminifera and some sponge spicules. The supposed radiolarians are from

.001 to .022 millimetre across and so had to be magnified from 1,000 to 2,300 times to enable an artist, who had never seen a radiolarian, to draw them. He copied what he saw. The smallest known Cambrian radiolarian is ten times the size of the largest of Cayeux's finds. Rust waxes sarcastic over them. He has never succeeded in getting more than 5 species of Palaeozoic radiolarians on 1,000 slides, whereas Cayeux got 41 species of his on one slide! Moreover, unlike Rust, Cayeux, although he got so many specimens on one slide, never succeeded in obtaining a cross section of any of them!

As to Cayeux's foraminifera, these are very minute, the largest having a diameter of barely .01 millimetre. Dr. Raymond points out, some of the specimens cannot possibly represent foraminifera, because the new chamber is not formed over the principal opening of the preceding one; for this reason he rejects them.

Walcott has described in Pre-Cambrian rocks what he believes to be the tracks and burrows made by some kind of worm. This view can neither be proved nor disproved.

Perhaps the most discussed supposed Pre-Cambrian fossil is that named Beltina by Walcott who discovered it in the Belt Series of North America. He found large numbers of this structure, which he deems to be mostly parts of the integument of an Eurypterid (a kind of Crustacean). Some he deems to be appendages of this supposed creature. Of these he has drawn a picture which is reproduced on p. 245 of Professor J. W. Gregory's "The Making of the Earth," where Beltina danai is described as " the chief member of the oldest known fauna!" This volume forms one of the

Home University Library of Modern Knowledge. Of these " fossils " Raymond writes : " The supposed test is extremely thin and in most cases without any regular outline. A very few fragments selected from thousands do remotely resemble Eurypterids. This may be said of four of the thirty-one specimens figured by Walcott." Raymond rejects the idea that these structures are part of an Eurypterid, nevertheless he considers them organic, but of algal origin. He is more favourably impressed by a Beltina found by Stuart Weller, which, he writes, " not only seems to have a definite outline but shows surface markings, and, being found in an arenaceous limestone, retains the original convexity. Its resemblance to an abdominal segment of the Mid-Cambrian Sidneya is very marked and there can be little doubt that it is a fragment of an Arthropod. Somewhat less satisfactory, but still fairly satisfactory, are the specimens figured from the Algonkian on the continental divide of Alberta. They may, I think be accepted as evidence of the presence of Arthropods in what may be part of the Belt Series. Unfortunately these finds cannot be fully accepted until checked by future discoveries."

Tillyard, however, writes (David and Tillyard's " Memoir of Fossils of the late Pre-Cambrian from the Adelaide Series " p. 90) : " They may well have been organic remains, but they appear to me to resemble torn parts of a marine alga rather than animal remains of any sort whatever ..... it seems clear that the Arthropod nature of Beltina is not yet established ..... I understand that a number of palaeontologists who have seen the actual fossils of Beltina consider that they may be the remains of a marine alga, this is cer-

tainly the impression formed on my own mind."

Curiously enough Professor Seward does not mention Beltina in his " Plant Life through the Ages !" As I have not seen the actual fossils, I am not able to judge them, but the illustrations are certainly not convincing.

My reasons for rejecting Beltina as a fossil are: first, as we have seen, inorganic structures occasionally asume very curious shapes.

A classical example of this is afforded by what appears to be the fossilised leather sole of a shoe in a Triassic rock. The sole " fossil " appears to be that of a child's shoe, size 13. The sole is completely silicified and is harder and more compact than the rock itself. It seems to have a double line of stitches, one line close to the outside edge and the other parallel at a distance of one third of an inch. The edges of the sole are rounded off smoothly as if cut by an expert cobbler. The right side of the heel seems to be worn more than the left. Does this mean that men who wore a modern type of shoe lived in the Triassic period or that the " fossil " is an inorganic concretion resembling a sole? Every one believes the latter. Professor W. D. Matthew writes " It is the most perfect piece of natural mimicry I have seen," and Dr. H. P. Whitlock describes it as the most deceptive specimen he has come across.

Secondly, Walcott has found thousands of these Beltinas—an *embarras de richesse.* If they be parts of an Arthropod, large numbers of this animal must have lived in the Pre-Cambrian Seas, and in that case, as numerous fossils of Trilobites, almost complete apart from the appendages, have been found, the same thing

ought to have happened in the case of Beltina in Pre-Cambrian rocks. The failure to discover anything approaching a complete fossil of this supposed animal disposes of its claim to be an animal fossil as effectively as Cayeux's failure to cut any cross-sections when making his slides disposes of his supposed radiolarians.

As Dr. Raymond makes no mention of the supposed Pre-Cambrian fossils described by David and Tillyard; apparently he does not accept them as such, or the rocks in which they were found as of Pre-Cambrian date. The Adelaide Series in which these fossils occur is believed by Howchin and others to be, not Pre-Cambrian but, Lower Cambrian. As the Upper part of the series is certainly Cambrian and there is no unconformity in the series, it is probable that all of it is Cambrian. These fossils are said to be algae, giant Eurypterids of very primitive structure, giant annelids having fin-like appendages, small pteropods and other forms. The illustrations of the supposed Eurypterids given by David and Tillyard in their volume are not at all convincing. In my opinion it requires considerable exercise of imagination to see in them resemblances to an Eurypterid.

The above descriptions may be rather wearisome to many readers, but in view of the great importance of the subject, I think them necessary in order to show how unconvincing are the supposed Pre-Cambrian fossils. Contrast the abundant clear-cut fossils found in the Cambrian with the indefinite, problematical Pre-Cambrian structures, which, except in the case of Beltina or Tillyard's supposed Eurypterid, are either supposed secretions, or marks made by animals or plants, or organisms that fit into no known group of animals or

plants. The wish seems to be father to the thought that these are fossils.

Nor is this all. Cambrian rocks containing abundant fossils occur in all parts of the world—viz. Wales, North and Central England, Scotland, Norway, Sweden, the Baltic Provinces, N.W. Russia, Poland, Bohemia, France, Spain, Portugal, Sardinia, Morocco, Siberia, Manchuria, China, Korea, Yunnam, India, Mesopotamia, Argentina, Bolivia, N. and S. Australia and Tasmania. This being so, I find it impossible to believe that abundant fossils would not have been found in Pre-Cambrian rocks in many parts of the world had the seas been populated when they were being deposited.

Thus, the total absence of undoubted fossils from Pre-Cambrian rocks and their abundance in the Cambrian must mean special creation at the beginning of the Cambrian Period, unless those who deny such creation can give a satisfactory explanation of the lack of definite Pre-Cambrian fossils.

Let us examine the various explanations suggested by evolutionists.

Formerly it was supposed that all the Pre-Cambrian rocks had become so metamorphosed and altered in the course of time that all the fossils they originally contained have been destroyed. We now know that, although many of the ancient rocks have been metamorphosed, a number have not. There exist in many parts of the world Pre-Cambrian sedimentary rocks, unmetamorphosed, undisturbed and eminently fitted to hold fossils. Examples of such are the Torridon Sandstones of Scotland, 8,000 feet thick, the Green Shales of Brittany, 17,000 feet thick, the Huronian

Series of Canada, 18,000 feet thick, the Belt Series of North America, 40,000 feet thick and the Cuddapah Series of India, 20,000 feet thick. Some of these Pre-Cambrian rocks are so perfectly preserved that they retain impressions made on their soft surface by drops of rain and ripple marks such as may be seen today on the sea shore.

The following are the more recent explanations of the lack of Pre-Cambrian fossils:

1. Before the Cambrian age all aquatic animals lacked shells or integument, or possessed shells so fragile and easily decomposable as to have left no traces in the rocks. In order to account for this strange state of affairs the following alternative suggestions have been made:

(a) In Pre-Cambrian times the calcium content of the sea was too low to admit of the secretion by organisms of calcareous shells. This is ruled out by the great quantities of limestones in Pre-Cambrian rocks.

(b) Professor Daly suggests that, although the sea contained abundant calcium, marine animals could not make use of it, because the only scavengers that existed to destroy dead organisms were bacteria which liberated sufficient ammonia to precipitate the calcium in the sea and so prevented animals using it to make shells. This theory, which involves the belief that in the immense period before the Cambrian during which animals lived no carnivorous animals existed, is refuted by the fact that the Trilobites, so abundant in the Cambrian period, were scavengers.

(c) Dr. Lane's theory is that the Pre-Cambrian Seas were acid, and this acidity prevented the formation of calcareous shells.

(d) Another suggestion is that all the Pre-Cambrian animals were vegetarians; in consequence shells were not needed for protection, and it was not until shortly before the Cambrian period that carnivorous Trilobites evolved and multiplied greatly, owing to unlimited supplies of defenceless food; then in self-defence many of the animals preyed upon by Trilobites developed shells!

(e) Professor W. K. Brooks' theory is that Pre-Cambrian animals lacked shells because they lived in the surface waters where shells would be detrimental on account of their weight.

(f) Dr. P. Raymond's theory is a modification of that of Brooks, viz. that all Pre-Cambrian animals were fast moving creatures and it was only at the beginning of the Cambrian that many adopted sessile or sluggish habits, and in consequence developed shells.

I find it exceedingly difficult to believe that scores of different kinds of animals—animals belonging to various Orders and Classes—with one accord suddenly began to secrete shells.

Further objections to all the above theories are, first, many of the Cambrian animals were so constituted as to render a shell or a test a *sine qua non* of existence. Secondly, even if all Pre-Cambrian organisms were devoid of shells, they ought to have left fairly numerous traces in the well-preserved sedimentary rocks, seeing that perfect impressions of jelly fish are not very uncommon in sedimentary rocks, and tracks, burrows and borings of soft-bodied animals are abundant.

2. Pre-Cambrian sediments contain no fossils because they were laid down in the sea, and Pre-Cambrian life was confined to fresh-water. Walcott and Chamberlin hold this theory. The former believes that life

originated in fresh-water; the latter thinks living organisms originated on land in the soil and migrated first to the rivers and then to sea, and did not reach the latter until the Cambrian Period.

As all Pre-Cambrian continental rocks have almost certainly been destroyed by erosion, it is not possible to prove that in Pre-Cambrian times the rivers and lakes did not teem with life. This theory, however, is not in accord with the opinion of many evolutionists that the salt content of blood is similar to that of the seas because life originated in the oceans. In my view a fatal objection to this theory is not only the great diversification of the known Cambrian fauna, but also some genera are widely distributed. Thus had the Trilobite, Olenellus, evolved in some river and migrated thence into the sea, it ought not to appear simultaneously in rocks so far apart as North America, Scotland and Siberia, and its ally, Redlichia, in China and South Australia. Nor should the Lower Cambrian coral, Archaeocyathus, make its appearance simultaneously in the Mediterranean and South Australia. Another fatal objection to this theory that in the Cambrian deposits fossils of Echinoderms are numerous. These animals are exclusively marine.

No living or extinct Echinoderm is known to have lived in fresh water. Is it credible that the Echinoderms and other groups that are exclusively marine have completely deserted fresh water in which some members of all the other groups of animals now exist?

3. Grabau's theory is diametrically opposed to the foregoing. It is that the Pre-Cambrian rocks are devoid of fossils because they were laid down in fresh water at a time when all life was confined to the sea!

This theory is easily disposed of. In the first place, as rocks exposed to the atmosphere are rapidly weathered away, it is highly improbable that a number of fresh-water formations, one 40,000 feet thick, have been preserved until the present time. In the second place, all the Cambrian rocks known to us seem to have been laid down in the sea. We have noticed (p. 115) how numerous and widespread these are. Is it credible that not a single rock laid down in the sea in the period immediately preceding the Cambrian has been preserved? Thirdly, the abundance of limestones and cherts in Pre-Cambrian rocks points to their being of marine origin.

4. Mr. Bradley imagines ("Parade of the Living" (1931)) that a period lasting millions of years separates the latest Pre-Cambrian rocks known to us from the earliest Cambrian. According to him, before any living organisms had evolved, "lands were lifted high above the seas" all the world over.

The erosion of these land masses to sea level occupied millions of years. While this erosion was proceeding the Cambrian fauna evolved in the sea, and, when the seas invaded the land, they brought with them the animals of which fossils occur in the Cambrian rocks. While there is no geological evidence in support of this theory, there is a piece of evidence fatal to it, viz. in certain localities there is no apparent unconformity between the Cambrian rocks and those underlying them. It is important to lay great stress on this fact, because assertions to the contrary are sometimes made in popular books. Thus, we are told in "The Science of Life" "A long gap of time seems to have elapsed between the laying down of the latest Proterozoic and of the earliest Paleozoic rocks. In no part of the world have sedi-

ments been found which bridge this gap, and that it was long is shown by the fact that wherever Proterozoic rocks are found with the rocks of the next period deposited on top of them, they have been first tilted and then worn down to a flat surface again."

The truth is that some of the Cambrian rocks rest on tilted earlier formations, but this is not always the case. Thus, Dr. A. H. Rastall writes (B. Vol. 10, p. 168): "The stratified rocks of the latest Pre-Cambrian times were very like those of the earliest Cambrian times, and in some cases there is little physical break between them." For example, in the Kimberley District of Australia the Lower Cambrian rocks rest conformably on the Pre-Cambrian Nullagrine Series of sandstones, limestones, conglomerates and lava flows.

In Adelaide, writes Dr. C. E. Tilley (B. vol. 2, p. 705), "owing to the difficulty of defining the base of the Cambrian it is uncertain whether this ancient tillite (the Sturtian) is Lower Cambrian or late Proterozoic in age." To come nearer home, Messrs. Gregory and Barrett write ("General Stratigraphy" p. 52): "The Cambrian beds of the Lake District...possibly should include the lower unfossiliferous part of the Skiddaw Slates."

5. Recently in a debate I asked Mr. J. McCabe what explanation he had to offer for the lack of Pre-Cambrian fossils. He replied that the Cambrian Period was preceded by a great Ice Age and no one knows where life was living at the time.

The above are all the attempts I have come across of evolutionists to explain the unfossiliferous nature of the Pre-Cambrian rocks. None of them holds water and some are fantastic. The mere fact that so many theor-

ies have been propounded shows the greatness of the difficulty to be met and the amount of attention that has been paid to it. Every one who holds any of these theories necessarily rejects all the others: I have no hesitation in rejecting them all.

In my opinion the unfossiliferous nature of the Pre-Cambrian rocks is fatal to the theory of evolution. Darwin appreciated this. He wrote in the first edition of "The Origin of Species": "But the difficulty of understanding the absence of vast piles of fossiliferous strata which on my theory no doubt were somewhere accumulating before the Silurian Epoch is very great... The case at present must remain inexplicable; and may be truly urged as a valid argument against the views here entertained." In the last of edition of his book he wrote, (p. 287): "Nevertheless the difficulty of assigning any good reason for the absence of vast piles of strata rich in fossils beneath the Cambrian System is very great...The case at present must remain inexplicable; and may be truly urged as a valid argument against the views here entertained."

This piece of evidence against the theory of evolution is far greater to-day than it was in 1859 on acount of our increased knowledge of the palaeontological record. The rocks cry out "Creation!"

CHAPTER XVI.

The Palaeontological Record

Dr. Davies is of opinion that the Palaeontological record is favourable to the theory of evolution. If he be right, either the record itself or our knowledge of it is very imperfect, because it has not yielded any of the fossils essential for the establishment of the theory. Before going into the question of the degree of imperfection of the record, I will notice Dr. Davies' criticisms of some statements made by me in this connection.

I am obliged to him for pointing out my mistake in spelling D'Halloy's name and in wrongly including him as an opponent of the theory of evolution. Dr. Davies expresses the opinion that W. C. Williamson should not have been included by me as an opponent of the theory. I confess that I have read very little of his writings. A perusal of his Lectures at Manchester in 1876-77 led me to place him among those opponents. He thus concludes his survey of the succession of life in the Palaeozoic Period: "Thus we see so far as we have accomplished our ascent from the lowest to the highest strata, race has been supplanted by race, generation has followed generation; occasionally we see evidence that seemed to indicate the existence of links connecting a departing race with another that succeeded it; suggesting the possibility of a gradual transition having taken place from lower to higher forms as years

rolled by. There are also broad general indications of an upward progress, shown by the introduction, from age to age, of animals having higher organisation than those which preceded them. But notwithstanding this, we are obliged to admit that, when viewed in minute detail, the rocks we have examined give but very limited support to the doctrine of evolution."

Dr. Davies objects to my assertion that Darwin was not a palaeontologist, because the latter was the author of a monograph on Fossil Cirripedes and collected fossils of mammals in the Pampas.

Dr. Davies is of course entitled to regard Darwin as a Palaeontologist, but I submit that Darwin would probably have repudiated such a title.

Depéret writes (" The Transformations of the Animal World," p. 37) : " It is important to notice that Darwin was very little of a palaeontologist. The only fossil animals he had personally studied, and that with a very real talent—the Cirripedes Anatifa, Balana, and Coronula-form a group degraded in their adult state by fixation or parasitism, and but little fitted on that account to make any interesting contribution to the history of evolution. Darwin also approached only with extreme diffidence the palaeontological, but most important side of the transformist hypothesis; yet his vast erudition enabled him to appreciate at its just value the weight of the objections raised against his theory by men of such authority as E. Forbes, Woodward, Murchison, Sedgwick, Pictet, Agassiz, Barrande, d'Archiac and many other determined partizans of the fixity of species, and of integral renewal of the fossil faunas. Compelled to answer these objections of fact, Darwin could only combat them by theoretical arguments, often

of mediocre value, but always ingenious."

I may add that two living palaeontologists, known to me, do not share Depéret's high opinion of Darwin's monograph on fossil Cirripedes. Darwin certainly had considerable knowledge of Palaeontology, but I cannot bring myself to regard him as a palaeontologist.

As regards living palaeontologists, the only ones known to me who openly oppose the doctrine of organic evolution are L. Merson Davies and P. Lemoine.

In view of the fact that the fossils found since Darwin's day do not fulfil the anticipations of evolutionists, the question arises: why is it that the great majority of palaeontologists opposed the theory of evolution seventy years ago, but do not do so to-day? The answer is given by Col. Merson Davies ("Evolution" Trans. Victoria Inst. vol. lviii (1926)): "If this doctrine of genetic connections and a common origin were put before us in the name of Philosophy, few objections could be opposed to it. It has all the merits of a finished creed. As a concept of nature it is unified, simple, and most comprehensive. It co-ordinates any number of facts in a most attractive manner, and strongly appeals—as we are repeatedly assured—to the 'modern mind.' Now that is a very strong argument in its favour as a philosophy; for every philosophy finds its basis, as Le Roy assures us, in a 'frame of mind.' Our own generation then, having acquired a frame of mind very different from that of our ancestors of a century ago—in that we are attracted by uniformitarian ideas as strongly as they were appealed to by catastrophic ones—evolution as a creed is undoubtedly better suited to ourselves that it was to them. Consequently it 'does not pay,' as they say, to

oppose evolution nowadays. Thus probably everyone has heard of Mr. H. G. Wells' writings on the subject, although Mr. Wells is no research worker ... but how many have heard of the works of a leading zoologist like Fleischmann, a scientist of European reputation, who flatly denied that evolution could be regarded as scientifically established? It is significant that no one has ever attempted the task of directly opposing Fleischmann; but he was thoroughly abused instead, and soon forgotten. When men of science find that the open expression of serious doubts upon the subject is treated after this fashion, it is natural that they incline to keep them to themselves. Nobody likes to be boycotted and reviled. Although, therefore, the great majority of scientific workers certainly do accept belief in evolution, we have no reason to suppose that they all do, even if we seldom hear of their openly opposing it."

I have cited elsewhere (M. pp. 95-104) other reasons for the ready acceptance of the evolution theory.

The quotation of Cuénot (D. p. 135) queried by Dr. Davies is to be found on page 370 of " L'Adapation " (1925).

Dr. Davies selects Dr. W. D. Matthew as an example of a zoologist who, it is difficult to believe, accepts evolution because he was taught it from boyhood as a dogma. Curiously enough I selected him (Trans. Vict. Inst. vol. lxiv p. 126) as an example of one who draws up animal pedigrees on what I consider inadequate evidence.

These, however, are trivialities. The truth or otherwise of the doctrine of transformism does not depend on a majority vote of scientific men.

Coming now to the palaeontological record. Since

the Cambrian Period 3 new classes of animals, 59 new orders and over 200 new families make their appearance in the rocks known to us. Nevertheless it is not possible to point to any fossil as a genetic link between any of these and an earlier group. This, I submit, must mean that these new families, etc. are not descended from earlier ones, or that those who have searched for fossils have been dogged by very bad luck, or that the palaeontological record, or our knowledge of it is exceedingly incomplete.

Let us consider the third of these alternatives.

Although Dr. Davies does not distinguish between our knowledge of the record and the degree of its completeness, I agree with much of what he says on these matters. He very rightly states a fact which I have never denied, viz. that the proportion of individuals of any species fossilised is " very small indeed." But it is surprising, that in the issue of " Nature " dated November 27th, 1937, there appeared the assertion over the initials "J. S. G." that for an animal to become fossilised is " almost a miracle," and, still more surprising that that Journal refused to publish a short letter of mine in which I gave statistics demonstrating that the statement in question is hyperbole.

The figures regarding the bivalve Spisula cited by Dr. Davies (E. p. 106) show that the probability of at least one member of a genus becoming fossilised during the existence of that genus is great.

It is estimated that 4,500,000,000,000 individuals of this creature exist in one area of 700 square miles. Dr. Davies asserts that if any appreciable percentage of the individuals capable of fossilisation had been preserved a famine in calcium carbonate and phosphate would have

set in far back in the geological record. He overlooks the fact that quantities of salts are carried by the rivers to the sea. The calcium carbonate and phosphate thus added to the sea would provide material for a large number of fossils. If during the whole period of the existence of Spisula only one millionth of the average population became fossilised, that would mean 4½ million fossils.

Thus, theoretical considerations indicate that the chances are great that some specimens of every genus having hard parts will become fossilised during the period of the existence of that genus.

In this matter, however, we can do better than theorise.

Some years ago it occurred to me that light might be thrown on our knowledge of the fossil record by ascertaining (1) the number of genera of any given class of animal of which fossils have been found at successive stages, and comparing these figures with those of genera now living, and (2) the percentage of the living genera of which fossils have been found.

I began on the second enquiry with the mammals now living in India and published the results (D. p. 147).

Here is the gist: 75 per cent of the terrestrial genera, 20 per cent. of the arboreal and 50 per cent. of the aquatic are known as fossils. Dr. Davies considers these percentages abnormally high, because the Siwalik deposits in which many of them are found are particularly favourable for jungle species.

On the other hand, as all these genera are still living the potentialities of their fossilisation are not yet exhausted.

In extending this enquiry to living genera of mam-

mals all over the world I collaborated with Mr. G. A. Levett-Yeats. On completion of the enquiry we submitted a paper embodying our results to the Zoological Society of London, of which we are fellows. The paper was rejected on the ground that our results led to no useful conclusions!

We then had the gist of them published in the Transactions of the Victoria Institute (vol. LXIV. (1932)) I here reproduce three short tables.

(The names of all living genera of which fossils have been found are given in the paper)

I

Type of Mammal.	No. of Genera now living.	Percentage known as fossils.
Volant (Bats)	215	17.67
Aquatic (Whales, Sirenians, Seals, etc.)	41	70.73
Land (i.e. all mammals other than aquatic and volant)	408	57.84
Total	664	45.63

II

Name of Continent.	No. of genera of land mammals now inhabiting it.	Percentage of these known as fossils.
Europe	48	100.00
Asia	145	70.15
Africa	134	49.65
North America	71	90.14
South America	86	72.09
Australia	48	45.83

III

The number of genera of non-volant land mammals known to have lived at various stages of the Tertiary and in the Quaternary and now living in Europe and North America.

Stage	Europe	North America
Basal Eocene	14	40
Lower Eocene	24	52
Middle Eocene	38	69
Upper Eocene	68	37
Lower Oligocene ...	80	58
Middle Oligocene ...	41	44
Upper Oligocene ...	43	57
Lower Miocene	52	51
Middle Miocene ...	59	35
Upper Miocene ...	81	52
Lower Pliocene ...	87*	42
Middle Pliocene ...	47	18
Upper Pliocene ...	45	28
Pleistocene	66	84
Now Living	48	72

*Includes fossils from the Maragha beds of Persia.

Prof. J. B. S. Haldane thus criticises Table III in his correspondence with Mr. Arnold Lunn ("Science and the Supernatural." p. 241): Dewar "lists 1,412 fossil genera spread over a time estimated at 50-60 million years. The average length of the 15 periods into which he divides this is thus between 3 and 4 million years. Such a period is a very long time. It is entirely ridiculous to compare a space of 3 million years with a single instant. There has been time within that period for forms different enough to be put in different genera to succeed one another repeatedly.

If a fair comparison is to be made between the existing animals and those of a past age, each of Dewar's

sub-periods would have to be split up into about a thousand periods of three or four thousand years each, i.e. as long as human history. You would find remarkably few fossil animals in any one of them. I should have thought that you would have seen through the trick of dividing the Tertiary group into the particular number of series which suits a given theory."

Even if Professor Haldane's estimate of the length of the Tertiary period be correct, his criticism is ineffective, being based on the supposition that all the fossils of each of the genera mentioned occur only in one of my periods; but this is the case of less than 20 per cent. of the genera. Most of them occur in more than two of my periods.

In such cases it does not matter whether each of my periods be split up into a thousand or ten thousand. If I see and speak to Jones on Monday and again on the following Saturday, I am prepared to go into a court of law and swear that Jones was alive on the intervening Tuesday, Wednesday, Thursday and Friday, even though I did not see him on any of these days. So with the fossils; if a genus be found fossil in my periods 1 and 3 but not in 2, I maintain that the genus was in existence in, at any rate, each of the thousand periods into which no. 2 is split up.

No amount of quibbling will do away with the fact that the above figures dispose of the myth that the fossilisation of an animal is "almost a miracle." The odds against any particular individual becoming fossilised are prodigious, but they are in favour of the fossilisation of at least one of the myriads that compose a genus during the period of its existence, and this is all that is required to record the existence of that

genus.

Dr. W. R. Thompson certainly does not exaggerate when he writes ("Science and Common Sense," p. 214): "It seems that there has been a rather general reluctance to face the facts which are more troublesome than is generally recognised."

The above statistics indicate (1) that the palaeontological record is almost complete IN THE CASE OF PLACENTAL MAMMALS OTHER THAN BATS (which are less liable than ground animals to fossilisation); (2) In the case of most of the continents our knowledge of the record is more or less imperfect. As new discoveries are made the percentages will increase.

It is important to bear in mind that the above statistics apply to living genera of mammals. Mammals of modern type appear comparatively late in the rocks known to us, i.e. in the late Cretaceous. In the case of Indian mammals I pointed out (D. p. 149) that all their fossils are in comparatively recent strata which have not been subjected to so much denudation as old strata, by way of caution against applying these figures to very ancient extinct animals. Dr. Davies' comment on this is (E. p. 116) that I seem "completely to misunderstand the relation of denudation to the geological record," and "the age of a formation is no criterion of the amount of denudation it has undergone."

Had I wished to score a debating point I could have agreed and asserted that the above statistics demonstrate the record to be practically complete in the case of terrestrial animals!

I agree that, when a sedimentary rock becomes ex-

posed to aerial denudation, the uppermost, that is the youngest, part is the first to be destroyed, but, despite this, I contend that in the case of a land deposit laid down in the Devonian and then submerged beneath the sea, and one laid down in Miocene and then submerged, the chances of the Miocene deposit being still in existence are far greater than those of the Devonian, since the latter has had so much more time in which to be elevated and subjected to denudation than the Miocene deposit. Thus, although the age of a formation is no criterion of the amount of denudation, the more ancient a land deposit the less are its chances of being preserved to this day, and this applies to the fossils in the formation.

It has been asserted that, if denudation continue at its present rate, in from one to five million years the whole of the British Isles will be reduced to sea level. I suggest that it would be more correct to say that all the land now above sea level will have disappeared, but, as the removal of this would lift a great weight off the underlying rocks, these would rise and land would still be above sea level, unless, of course, in the meantime great earth movements had caused some continents to become seas and some seas to become continents.

My contention is that no sedimentary rock can exist for an immense period of time unless it becomes submerged beneath the sea and there protected from erosion.

Therefore, if the continents and oceans have all along remained permanent, the record of the earlier land fossils must be very imperfect.

The fact that all the known Cambrian, Ordovician and earlier Silurian rocks appear to be marine suggests

that the land deposits of those periods have been destroyed by denudation. Were the continents well stocked with animals and plants during these periods? In view of the great diversity of the Devonian land flora the evolutionist is compelled to believe that land plants were abundant, at any rate during the Silurian. The creationist is under no necessity to believe this. He depends on the fossil evidence, and, as a few fossils of land plants have been found in the marine Silurian and Ordovician rocks,* there can be no doubt that land plants existed during these periods. In this connection the paper by Drs. L. W. Bailey and G. F. Matthew entitled "Some Problems of New Brunswick Geology" (Trans. Roy. Soc. Canada, Series III. vol. 12 (1918-1919)) is of great interest. These two geologists assert that *all* the geological data point to the deposits known as the Fern Ledges of Carleton being of Silurian date. The fossils they contain include those of highly developed plants such as Dadoxylon and Araucarites and of air-breathing animals such as neuropterous types of insects, myriapods and snails, as well as footprints of amphibia. Palaeobotanists, on account of the highly developed plants, assign these beds to the Carboniferous age and refuse to accept the findings of the geologists regarding the date of the deposit.

Zoologists agree with the botanists, with the result that there is no mention of the paper of Bailey and

* There is ("Science" vol. 85, Supplement p. 12 to issue of May 14th, 1937) a reported find of what is said to be the shoot of a land plant in the Black Oil Cambrian Shale of Sweden. This was identified by Dr. W. C. Darrah by means of a new technique enabling very thin sections to be cut. He found what he believes to be spores barely visible to the naked eye, "marked by a small three-pointed star or tetrad scar, a characteristic of early growth stages in higher plants but not found in primitive water plants." Another characteristic of land spores was also observed, viz. a wax coating. In default of confirmation this fossil must be classed as doubtful.

Matthew in any recent book I have consulted. I came across the paper by chance about six months ago.

Recently Miss Hendricks discovered some fossil wood (Dadoxylon hendricksi) in two Cornish deposits assigned respectively by the Geological Survey to the Lower Devonian and the Ordovician. This wood consists of secondary xylem, like that seen in living conifers. The existence of vascular wood of such an advanced type in the Ordovician Period is not in harmony with the doctrine of evolution. In consequence botanists have relegated these two formations to the Middle Devonian. Professor Seward (S. p. 126) somewhat naively writes; " This discovery affords a good example of the employment of fossil plants as indices of geological age; on botanical grounds the Cornish rocks are referred to a Middle Devonian horizon; on purely geological evidence they are assigned to Lower Devonian and some of them to the Ordovican Period. We cannot, however, confidently assert that no trees with stems of the Dadoxylon type existed before the middle of the Devonian Period." The above are striking examples of the way in which modern biologists are dominated by scientific doctrine.

The sudden appearance of flowering plants in great variety in the Lower Cretaceous rocks is another indication of the imperfection of the geological record of the land organisms living before the middle of the Mesozoic Era. Flowering plants were probably abundant throughout that Era and possibly in the Palaeozoic; nevertheless only a few fragmentary fossils of them have been recorded from Jurassic deposits.

The fossil found in a coal ball in a Carboniferous deposit, described as a monocotyledonous stem and

named Angiospermophyton americanum by Dr. A. C. Noe (Jour. Geol. 1923) is said by Professor Seward (S. p. 522) to be the leaf stalk of a pteridosperm (Medullosa) which bears considerable resemblance to the stem of Indian Corn (Zea mais).

Provided, then, that considerable areas of the continents have all along been above sea level, it would seem that the geological record, while almost complete for the greater part of the Tertiary Period, is far from complete in respect of the land organisms of the Mesozoic and Palaeozoic Eras. And, as regards our knowledge of the record, this would appear very imperfect in the case of all organisms confined to parts of the oceans far from land, because the marine rocks known to us seem nearly all to have been laid down at no great distance from land, being mostly composed of terrigenous matter, and there are limits to which this can be conveyed by water.

These rocks have been far less exposed to weathering than have the land rocks. Thus, PROVIDED THE CONTINENTS HAVE REMAINED ABOVE SEA LEVEL, apart from periodic incursions and recessions of the oceans, the geological record, as regards land organisms seems to be nearly complete from, let us say the Oligocene, before which it becomes progressively less complete. In the case of organisms living in the sea, the record seems to be nearly complete as regards animals having hard parts, but our knowledge of it is confined to those of which the fossils occur in rocks laid down within a few miles of the shore.

If, on the other hand, the continents and oceans have not been constant, then the record is more complete and our knowledge of it is greater.

In the present state of knowledge it is not possible to say whether or not the continents and seas have frequently changed places in the past, but it seems that the balance of the evidence is against this.

If the relative positions of these have not changed, it is, of course, open to the evolutionist to say that the fossils do not disprove his theory, because all major evolution took place in the bracing air of the mountains or in the oceans far away from land, and that after the animals and plants had migrated from these localities, they have undergone little or no modification, also that evolution in the major sense is no longer going on. The former proposition cannot be disproved, but it seems highly improbable. I repeat that the known fossils appear to be witnesses hostile to the concept of evolution.

CHAPTER XVII.

The Origin of Families.

I agree with Dr. Davies in the matter of the differences of opinion among taxonomists as to what constitutes a natural family. This fact alone suffices to prevent me from asserting that the family, or any other taxonomic group, constitutes the unit of creation.

Nevertheless, Dr. Davies's criticism of my views is based on the assumption that I deem the natural family to be the unit of creation. (See p. 8). In my "Difficulties of the Evolution Theory" I set forth briefly Vialleton's theory that the greater groups of the animal kingdom down to sub-orders or super-families can only have originated from earlier ones by a special sudden change at an early stage in the course of the development of the embryo, which absolutely excludes the process of phylogenetic development required by the doctrine of evolution. I then suggested that, in order to make theory fit known facts, it is necessary to adopt a provisional hypothesis such as that of Vialleton, adding (D. p. 158) "Such a course would certainly stimulate research. It would lead to systematic efforts to discover the extent to which organisms are proved to have undergone evolution in the past. When this is determined we shall know precisely what evolution has to account for." That was written in 1930. Since then I have begun to suspect that the units of creation

may be more restricted than those suggested by Vialleton. A paper of mine read to the Victoria Institute in 1932 contains the following passage: "The proposition I submit for your consideration is the changes that have been effected gradually in animals are strictly limited and do not transgress the limits of the natural family." I did not say "the family of modern systematists." I also asserted that it is not possible with the known fossils to constitute a single phylogenetic series linking a member of any mammalian family with a member of any other family. I am now inclined to think that this assertion may be extended to animals other than mammals.

This is to assert, not that the family is the unit of creation, but that there is no fossil PROOF that any family is derived from an earlier one. I might perhaps venture to go a little farther. There does not appear to be any proof, experimental or fossil, that all the species which constitute a family are derived from a common ancestor. For all we know at present, each family may have been created, not as a single pair but as a collection of pairs, each a little differently constituted, and each the ancestor of a lineage which has undergone more or less modification in the course of time.

On the other hand, should it later be demonstrated, either by experiment or an undoubted genetic series of fossils, that a member of a natural family can be, or has been, converted into a member of what all agree to be a different family, then, but not until then, it will have been demonstrated that the units of creation are greater than the natural family.

Dr. Davies disagrees with me; in his view there exists

fossil evidence—perhaps I should say proof—that families have originated by a process of evolution from other families. He adduces evidence in support of his view. Let us examine this.

The Equidae or Horse Family.

Dr. Davies devotes 14 pages of his book in endeavouring to demonstrate that the Equidae are derived from an earlier family; nevertheless he does not venture to name a non-equine ancestor of the Equidae. He gives what may be described as circumstantial evidence. He expresses the opinion that Eohippus, which many suppose to be an ancestor of Equus, is less like Equus than it is like Homogalax (Systemodon), Lophiodon and Hyrachyus, which no one includes in the Equidae and which are deemed to belong to other families, and he argues (E. p. 64) "the acceptance of Eohippus as ancestor to Equus seems logically to involve the broad relationship of all the Perissodactyls. He further expresses astonishment that I should " strain at an Acila and swallow an Eohippus." I will deal first with the second statement. I neither strain at the one nor swallow the other. I merely point out that there is no fossil proof that Acila and Nucula, two genera of the family Nuculidae, are derived from a common ancestor, although they both have been long in existence. I do not admit that Eohippus is an ancestor of Equus, nor do I deny this. I wrote (D. p. 107) of Eohippus and other supposed later ancestors of the horse: " so far no fossils have been found connecting them with the horse of to-day. Some of them may be actual ancestors of the living Equus, but that has yet to be proved." To this

statement I still adhere. I do not think it impossible that, as environmental conditions changed, members of the Equidae increased in size, that their teeth developed high crowns and their central toes grew in magnitude at the expense of the others; the known fossils suggest that this may have happened.

As regards the derivation of the Equidae from an earlier family Dr. Davies is commendably cautious. He thinks that there is much to be said for the classification adopted in 1889 by Lydekker and Nicholson in which are included in one family, styled the Lophiodontidae, what I deem to be a heterogeneous collection of Eocene mammals, viz. Eohippus, Epihippus, Orohippus, Colodon, Lophiodon, Helateles, Homogalax, Hyrachyus and Tetraclaenodon. If this classification be accepted, then the fossils indicate that the Equidae are derived from an earlier family and Dr. Davies has made his point. But, in my view the Lophiodontidae do not constitute a natural family, and I believe that the majority of those who have considered these Eocene mammals will agree with me.

Prof. W. D. Matthew, whom Dr. Davies mentions with approval, would divide this assembly into no fewer than three super-families—the Hippoidea, the Tapiroidea and the Rhinocerotoidea; while Prof. Romer, who also is commended by Dr. Davies, divides these animals into three sub-orders. It is true that Romer expresses the opinion ("Vertebrate Palaeontology," p. 323) that Eohippus "without doubt is very close to the roots of the whole Perissodactyl stock," but opinions, even of evolutionists, are not proof. Neither Romer nor anyone else, so far as I am aware, has dared to name any fossil as the ancestor of the Perissodactyls.

We must bear in mind that in few of these early Eocene mammals is the full skeleton known, and the parts that are known exhibit considerable diversity. All these genera appear to have the same number of upper teeth, but this is not so in the case of the lower teeth. Thus, Colodon has two incisors on either side, most of the others have three. Colodon and Lophiodon exhibit only three premolars, the others have four.

In some of the genera the lower molars have oblique transverse crests, in the others these crests are at right angles to the axis of the crown, in yet others they are V-shaped.

As regards the upper jaw the last two premolars of Helateles exhibit a supplementary crest, in Colodon the last three display this feature.

Clearly then there is no fossil *proof* that the Equidae are derived from any earlier family. This, I think, is equally true of the Tapiridae and Rhinocerotidae. Dr. Davies, however, has drawn up a pedigree in which he shows Homogalax (Systemodon) as the common ancestor of these two families. I doubt whether many evolutionists accept this.

Professor Romer writes (*op. cit.* p. 330) : Homogalax is " close to the ancestry of the Tapiridae, if not the actual ancestor," but he does not consider this genus to be ancestral to the Rhinocerotidae; according to him, the latter group are derived " from some of the smaller and more primitive rhinoceroses of the Eocene, and through them from the primitive Perissodactyl stock," a guarded guess.

Dr. Stamp writes (B. vol. 8, p. 632) : " Tapirs were foreshadowed by Systemodon and similar forms (Palaeotherium, Palaplotherium); the peccary like Hyraco-

therium was the forerunner of the horse. Hyrachyus was a primitive rhinoceros." These are all guesses.

As regards the slenderly built Hyrachyus, Dr. W. K. Gregory writes (B. vol. 17, p. 530): "none of the known species of Hyrachyus were directly ancestral to the line running to the later rhinoceroses." That the hornless Aceratherium is ancestral to Rhinoceros (which is known to have existed as early as the Lower Miocene) seems highly improbable, and Depéret does not admit it.

Thus, the best that can be said for the alleged ancestors of the Tapiridae and the Rhinocerotidae is that, if these be derived from earlier families the genera cited are more likely than any others known to us to be their ancestors, or close relatives of these. I have no hesitation in asserting that it has not been proved that these supposed ancestors are in fact such.

To return to Eohippus, Dr. Davies takes exception to my assertion that it is as clearly a horse as a pouter pigeon is a pigeon. I am inclined to think he is right. No one has seen Eohippus in the flesh, and the making of reconstructions from skeletal remains is a hazardous business.

However, in making the above statement I am in respectable company; the pictures of Eohippus that appear in the books of Vialleton, Osborn and Lull all bear the unmistakable stamp of the horse. I am surprised at the statement of Dr. Davies that Equus is not known before the Pleistocene. It occurs in the Siwalik Beds which were deemed to be Miocene before a fossil of Equus was found in the upper part, which was then promptly relegated to the Pliocene! Equus also occurs in the Paloduro and the Blanco beds of North America,

which are, I believe, generally deemed Pliocene. Possibly Dr. Davies accepts Dr. Leaky's definition of Pleistocene, viz. " if a member of one or more of the genera Elephas, Bos and Equus (however primitive) occur in a deposit, that deposit is Pleistocene rather than Pliocene." ("Adam's Ancestors," p. 26).

For the benefit of those who have not made a special study of zoology it is necessary to lay stress on the fact that the pedigrees of the horse set forth in the books are full of gaps.

Take the pedigree of Dr. Davies, here reproduced. (Fig. 1).

It is not possible to arrange a chronological series of the known fossils of Eohippus, each member of which is a little more like Orohippus than its predecessor, until we reach a fossil of which it is difficult to say whether it be an Eohippus or an Orohippus. This is equally true of all the other terms of the series.

As the anatomical characters of Orohippus are in some respects intermediate between those of Eohippus and Epihippus, and the beds in which the fossils of Orohippus occur are deemed to be of later date than those containing Eohippus and earlier than those containing Epihippus, some zoologists infer that Orohippus is a descendant of Eohippus and an ancestor of Epihippus, and so with the other terms of the supposed genealogical series. Thus, the whole pedigree is based on inferences drawn from fossils.

There might be a strong presumption that these inferences are correct if the genera set forth as a genealogical series were the only horses of which fossils are known. But this is not the case. Fossils of other extinct genera of horses are known intermediate between

Eohippus and the modern horse as regards both time and anatomical characters.

The method of those who construct horse pedigrees is to select from any given bed the horse genus which seems to be the best intermediary between the genus from the next older and younger beds, and insert this in the line of ancestors. Thus, the pedigree of Dr. Davies (Fig. 1) is composed of a selection of American genera.

TABLE I.

AMERICAN FOSSIL HORSES (W. D. MATTHEW. Q. Rev. Bio. (1926))

Period	Name of Bed	Names of genera of fossil horses
Pleistocene	Glacial and Interglacial	(10) Equus
Pliocene	Blanco	(9) Plesippus
Lower Pliocene and Upper Miocene	Nebraska, Texas, New Mexico, Montana	(8) Protohippus, Pliohippus, Hipparion
Middle Miocene	Deep River	
Lower Miocene	Harrison and John Day	(7) Merychippus
Upper Oligocene	Titanothere	(6) Parahippus
Middle Oligocene	White River Oreodon	(5) Miohippus
Lower Oligocene	Protoceras	(4) Mesohippus
Upper Eocene	Uinta	(3) Epihippus
Middle Eocene	Bridger	(2) Orohippus
Lower Eocene	Wasatch	(1) Eohippus

(Note. As the above formations are in various localities their ages are largely determined by the fossils they contain.)

In his pedigree Dr. Davies includes Nos. 1 to 7 of the genera named in Table 1, Protohippus as No. 8, and omits Plesippus, which he may well include in Equus.

Dr. W. D. Matthew has drawn up a very different pedigree. (Fig. 2.)

It will be observed that Dr. Matthew differs from Dr.

Fig. 1
PEDIGREE OF THE HORSE (after Morley Davies)

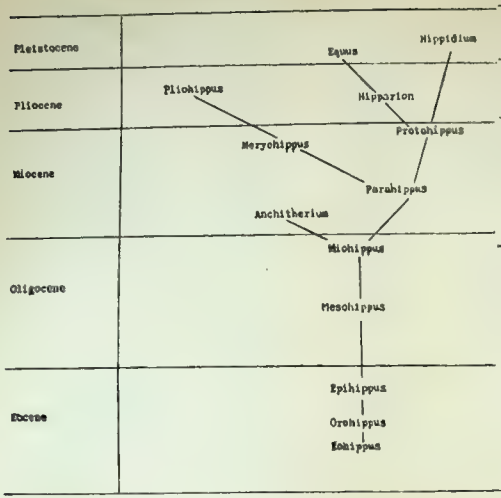
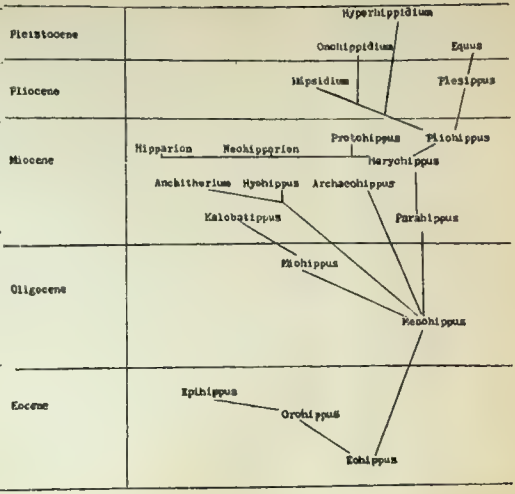


Fig. 2
PEDIGREE OF THE HORSE (after W.D. Matthew)



Davies in the following matters. (1) He does not deem Orohippus and Epihippus to be ancestral to Mesohippus, hence in his pedigree there is a great gap between this last and Eohippus. (2) He does not consider that Miohippus is ancestral to Parahippus, and thus leaves a considerable hiatus between Mesohippus and Parahippus; in other words he inserts one intermediary between Eohippus and Parahippus, while Dr. Davies inserts four. (3) He regards Pliohippus as intermediate between Merychippus and the modern type of horse, regarding Protohippus as a side line. Dr. Davies does the reverse.

As I have seen neither the fossils concerned nor the rocks in which they occur, I am not able to judge as to the relative merits of the above two pedigrees. But it is clear that one must be incorrect; both are hypothetical.

Dr. Matthew in his table does not actually connect Equus with any of the other genera, but in his later note on fossil horses (Enc. Brit. Vol. XI, p. 757) he says that Equus is probably derived from Plesippus.

The above considerations make it clear that the most that can be said for a pedigree deriving Equus from Eohippus is that it is not contradicted by the fossils, indeed they lend slight support to it. Clearly then to assert positively that the modern horse is descended from Eohippus is at the best to state as a fact that which is merely not improbable. This does not prevent very many evolutionists writing as though the descent were proved.

It is this that elicted the rebuke of Depéret (" Transformations of the Animal World" p. 105) : "The supposed pedigree of the Equidae is a deceitful delusion,

which simply gives us the general process by which the tridactyl hoof of an Ungulate can transform itself, in various groups, into a monodactyl hoof, in view of an adaptation for speed; but in no way enlightens us on the palaeontological origin of the horse."

Depéret is probably right in thinking that the fossils show how a tridactyl foot may be converted into a monodactyl one. But certain considerations indicate that this statement is premature.

1. Although it is true that all the earlier horses known to us have more than one toe, and the earlier the horse the greater the number of toes, we must bear in mind that the rocks are to some extent dated by their horse fossils (vide p. 143). Thus, if it be ruled that every formation that holds a one-toed horse cannot be earlier than Pleistocene, clearly all supposed older rocks must contain horses having more or less well-developed lateral toes.

One is inclined to ask, are all the rocks that contain horse fossils correctly dated, do the horse fossils really show this gradual and orderly reduction of toes?

2. The extinct family Litopterna is believed by evolutionists to resemble the Equidae in the loss of toes. But in them the one-toed genera appear early, thus Thoatherium of the Miocene is more completely one-toed than the modern horse, since it lacks splint bones; on the other hand the latest fossil of the group, Macrauchenia of the Pleistocene, has three well-developed toes. In Miocene and Pliocene times there lived one-toed Litopterna, and three-toed forms having the lateral toes ill-developed and those with them well-developed. In their case, then, if the reduction of toes did occur gradually, we must assume a number of lineages in which the

loss took place at greatly varying rates.

3. Although the camels have a long fossil history, the earliest as well as the latest exhibit only two toes. If, then, they are descended from five-toed ancestors, all except the middle pair seem to have been completely lost before the Later Eocene.

4. It is possible to form a pedigree deriving Equus from an ancestor having four front toes and three hind ones composed of genera that lived in Europe, almost all different from the American. The Europe series begins with the Lower Eocene Hyracotherium (which some would include in Eohippus), followed by Pachynolophus, then by Anchitherium, then Hipparion and finally Equus. This series is not quite so complete as the American. But it must mean, if it be in any way genetical, that Equus has evolved independently in Eurasia and in North America.

5. Since the Palaeocene Period 37 new families of mammals make their appearance in the rocks of Europe and North America. There is no fossil proof of the origin of any of these from any other known family. They seem to be immigrants from the north; they make their appearances at various times. From this it is not unreasonable to suppose that the various types of horses found fossil in Europe and North America may likewise have been immigrants from the North, and that the one-toed species were the latest immigrants. It may be that all along there have existed one-toed horses, those having the lateral toes well-developed and those having these ill-developed.

Lest it be thought that I am unduly meticulous in the matter of the supposed evolution of the Equidae, let me submit that my attitude is scientifically correct.

To the objection that, the state of the geological record or our knowledge of it being what it is, if a rigid proof be demanded, it is impossible to prove the truth of the theory of organic evolution, the answer is that it is not the business of the biologist to prove any theory, much less adopt it as a creed without proof. His business is to ascertain facts, co-ordinate them and endeavour to explain them. It is not only legitimate, but useful, nay almost necessary, to frame hypotheses in order to try to account for the phenomena presented by the animal and vegetable kingdoms. By all means formulate hypotheses. I contend, however, that it is essential for the welfare of biology that unproved hypotheses be not accepted by biologists as established truths; that it is most injurious to scientific progress if facts that do not fit in with theory be ignored, or distorted in order to bring them into line with this.

Retain the theory that all the Perissodactyls are descended from a common ancestor having five toes, but do not treat this as an established fact, and do not base the ages of fossil-bearing rocks on the assumption that it is established. Recognise that, although Equus may be descended from Eohippus, it is, in the present state of knowledge, unscientific to assert this in popular books, or to state in a book written for young students: "At the beginning of the Tertiary Era horses had five toes, not one"?*

* This assertion occurs on p. 291 of "Biology, An Introduction to the Study of Life" (1934) by Prof. H. Munro Fox, (Cambridge University Press).

The Halicoridae or Sea-Cow Family.

When criticising (D. p. 59) the series of fossils arranged by Professor O. Abel in support of the theory that the Sirenia (Halicoridae) and the Ungulata are derived from a common ancestor, I gave seven reasons for my belief that Abel's series is not a genetic one.

1. The pelvis of the Manatee does not fit into the series and bears no resemblance to that of an ungulate. Dr. Davies meets this by saying that, if we assume that in the Manatee two of the three bones of which the pelvis was originally composed have almost disappeared while the ischium has retained its original size, then what remains of the pelvis of the Manatee bears an unmistakable resemblance to the ischium of Moeritherium. If we assume that the reduction of the pelvis took this peculiar course, which, by the way, is not in accordance with the views of Kraus, the pelvis of the Manatee fits fairly well into Abel's series; but then the question arises: why has the ilium disappeared almost completely in the Manatee while it has persisted with comparatively little reduction in the Dugong?

2. There is no proof that the later members of Abel's series are descended from the earlier. Save admitting that Eotheroides (Eotherium) is more probably a cousin than a parent of Eosiren, Dr. Davies does not deal with this point. Let me elaborate it. As Dr. Davies has published (E. p. 89) illustrations, drawn to scale, of the hip-girdles of the members of Abel's series, the reader is in a position to judge of the probability of the series

being a truly genetic one. He will at once notice the peculiar changes that are supposed to have taken place in the size and shape of the pubis, in the proportions, shape and curvature of the ilium and in the situation of the acetabulum.

The rather pointed pubis of Moeritherium is supposed to have become larger and less pointed in Eotheroides, to have reverted to something like its original form in Eosiren, to have retained this form in Halitherium, and then almost to have vanished in Metaxytherium.

The upper part of the ilium of Moeritherium has a slight backward bend; the ilium of Eotherium is club-shaped and straight, that of Eosiren is shorter and tapers at the end, that of Halitherium is longer, more slender and has a slight forward bend, while that of Metaxytherium is stouter and ends in a blunt expansion.

The acetabulum occupies a posterior situation in Moeritherium, a median one in Eotheroides, an anterior one in Eosiren, a posterior in Halitherium and a median in Metaxytherium. Moreover, having thus migrated forwards and backwards, it disappeared in the Dugong!

The above is a strange course to be taken by a bone which has become useless and is disappearing. I submit that the particular form of the bone in each genus is an adaptation to the functions it performed.

3. Eosiren lived too shortly after Eotherium to allow sufficient time for the alleged conversion. With this Dr. Davies is inclined to agree. Thus, this link of the pedigree falls out.

4. It is improbable that the obturator foramen disappeared earlier than the acetabulum. Dr. Davies suggests that the latter may have persisted because the femur articulated with it. I agree.

5. The gradual transformation of a land mammal into a sea-cow or whale appears to be impossible. (See p. 21)

6. There are no known animals, living or extinct, anatomically midway between these aquatic mammals and land mammals. To this objection Dr. Davies replies (E. p. 93): "The absence from the geological record of transitional forms between the Sirenia and the land mammals from which they should be derived is admittedly 'a difficulty of the evolution theory.' The same is the case with the Cetacea and the marine reptiles (Chelonia, Ichthyosauria, Plesiosauria): in all these cases, although the earliest known fossils are nearer to the supposed ancestral land-animals than the later ones, there is a wide gap left." I assert that the earliest known turtles and the earliest known cetaceans were fully adapted to an aquatic life. It is true that the head and teeth of the earliest known cetacean, Zeuglodon, are more like those of a land mammal than are those of any living whale, but there is no reason why a marine mammal should not have been created having such a head.

Dr. Davies continues; "In the case of flying vertebrates I shall suggest (Chap. VI.) that the earlier transitional forms were tied to an arboreal life. It may be that an analagous explanation must be accepted for these aquatic mammals—that in their early phase they were confined to fresh waters and that there are no fresh-water deposits known of the place and period of their early evolution. In particular there is good reason to believe that both Cetacea and Sirenia originated in Africa and no fresh-water Upper Cretaceous or Tertiary deposits earlier than the Upper Eocene are yet known there."

This might account for the absence of fossils ancestral to the Cetacea and Sirenia, but it does not apply to the turtles, the Ichthyosaurs, the Plesiosaurs, and particularly the Order or Sub-Order of sea-serpents known as the Mosasauria or Pythonomorpha, which appears suddenly in the Upper Cretaceous, in the shape of four families, in North America, Europe, North, West and South Africa, and New Zealand.

A very serious objection to the doctrine of transformism is the total lack of fossils between the animals of which the gradual evolution seems impossible and their supposed generalised ancestors.

Dr. Davies makes no attempt to explain why the Right Whale, the Cetacean most highly adapted to an aquatic life, has inside the body, bones corresponding to hind leg bones, while some Cetaceans lack these.

In reply to Dr. Davies' assertions that Eotherium has a pelvis exhibiting "the normal characters of a land-animal" and "differs little from the pelvis of the earliest known Proboscidian, Moeritherium of the Upper Eocene," let me quote Vialleton (M. p. 377) : "The pelvis of Eotherium is represented in a vertical position, whereas, if it be truly constructed like that of an ungulate, it ought to be very oblique, at 45 degrees, which greatly changes conditions, because if it have this inclination and the same muscular relations as in the ungulates, it would no longer be in accord with the rest of the skeleton of a sirenian. In effect in this last the sacral region is very short (a single vertebra), the chevron bones begin from the second caudal vertebra : with the length of the ilium and the inclination of the bone, a large part of the pelvis, perhaps all of it behind the acetabulum, would be placed at the level of or above

the first caudal chevron, the natural orifices would thus be situated farther back than usual and the organs that abut on them would be singularly incommoded by the muscles of the tail. Further it should be observed that the obturator foramen is relatively small, the ischium is already abnormally large, and lastly the pubis does not appear to be at all bent inwards to form a symphysis; so that it may be asked whether the comparison of the pelvis of Eotherium with that of an ungulate is quite justified, and whether we have not here to do simply with one of the aberrant forms of the pelvic bones which Kraus shows to be different from one another in the single genus Manatus. Indeed, as he shows, there is as much difference between the pelvis of a male and that of a female of the same size as there is between those of Eosiren and the Dugong."

Vialleton concludes: "In face of these observations may not one be permitted to ask if there is not merely a purely superficial resemblance between the pelvis of Eotherium and that of the proboscidian ancestors from which it is supposed to be derived?"

The above is necessarily very technical, but the general reader will be able to appreciate that there is no proof that Eotherium is closely related to Moeritherium, that it is an ancestor of the living Sirenia, or that the other members of Abel's series constitute a true lineage. In short, it is not proved that the Halicoridae are derived from an earlier family.

SOME MOLLUSCAN FAMILIES.

The other families of which Dr. Davies tries to established the evolutionary origin are those of molluscs. I

submit that these animals are peculiarly unfitted to afford help in settling the evolution-creation controversy.

Their fossil remains are confined almost entirely to their shells, which are the houses they secrete for themselves. The outward form of the shell, particularly that of a sessile animal, is affected by the nature and amount of food available, the chemical content of the surrounding water, its temperature, degree of muddiness, etc. and the density of the population.

Thus the shell of one and the same species may assume a great variety of form. An excellent account of the variability of the shells of molluscs, is given by Dr. Cooke in the second volume of the "Cambridge Natural History," illustrated by a picture (here reproduced) showing the shells of 19 varieties of the dog-whelk (Purpura lapillus) living in different parts of the British Isles. These shells differ in size, shape, proportions, relative size of opening, length of spire, and thickness and roughness of the shell. Nor is Purpura unique. F. P. Marrat is of opinion that all of the 150 described species of another genus of dog-whelk—Nassa belong to one and the same species!

There can be little doubt that a great many of the described species of molluscs are not entitled to this rank. Hazay was able to rear Limnaea peregra from L. ovata, and ovata from peregra, by the simple device of placing the eggs in still or running water. If, in course of time, conditions of life in a locality change, the shells of the molluscs able to exist in the new conditions are liable to become modified.

A classic case of this is afforded by an Upper Miocene deposit at Steinheim, generally believed to have been laid down in a lake of which the water gradually became

NINETEEN SPECIMENS OF PURPURA LAPILLUS, THE DOG-WHELK, FROM GREAT BRITAIN.

1. *Felixstowe, sheltered coast.*

2 *and* 3. *Newquay, on veined and coloured rock.*

4. *Herm (Channel Isles), rather exposed.*

5. *Solent, very sheltered.*

6. *Land's End, exposed rocks, small food supply.*

7. *Scilly, exposed rocks, fair food supply.*

8. *St. Leonard's, flat mussel beds at extreme low water.*

9. *Robin Hood's Bay, sheltered under boulders, good food supply.*

10. *Rhoscollyn, on oyster bed, four to seven fathoms (Macandrew),*

11. *Guernsey, rather exposed rocks.*

12. *Estuary of Conway, very sheltered, abundant food supply.*

13 *and* 14. *Robin Hood's Bay, very exposed rocks, poor food supply.* 14 *slighty monstrous.*

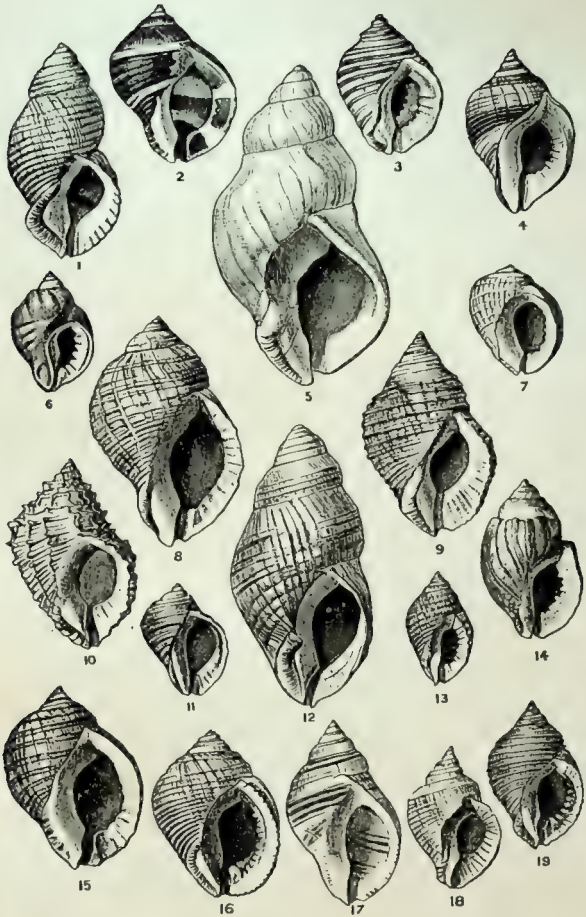
15, 16 *and* 17. *Morthoe, rather exposed rocks, but abundant food supply.*

18. *St. Bride's Bay.*

19. *L. Swilly, sheltered, but small food supply.*

Note that all the above shells belong to one and the same species. If this dog-whelk were extinct and known only by its fossil shells, how many species and genera would these be deemed to represent?

(*Reproduced with the permission of Macmillan & Co Ltd., from "The Cambridge Natural History."*)


1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19

warmer and more saline owing to the intrusion of hot springs.

Among the molluscs inhabiting this lake was the snail, Planorbis multiformis, so named on account of the many varieties of it found in the Steinheim deposit. The genus still persists, and several species occur in ponds in this country and are popularly known as ram's horns or trumpet-snails.

Hilgendorf, who made a special study of these Steinheim Planorbis, described no fewer than 19 varieties, or species. Some of the shells are flat discs, the coiling being all in one plane, the diameter of the chamber of others increases with the age of the animal, the shells of others assume a spiral form, some are like the shell of the whelk, in shape. Hilgendorf found in the lowest layers of the deposit only two varieties, both having flat disc-like shells, but in the higher zones less-flat forms appear and, two-thirds of the way up, cone-shaped shells. Later flat shells reappear, some of which are very like the shells in the lowest layer. Hilgendorf called these later flat forms P. revertens, deeming the earlier changes to have become reversed in them.

The differences between a flat-shelled form as P. tenuis, a slightly spiral such as P. elegans and a cone-shaped one such as P. trochiformis, are so great that some might deem them to belong to different genera.

Hilgendorf believes the majority of these Steinheim shells to be derived from the form he styles P. aequeumbilicatus, being doubtful about four of the varieties, viz. P. parvus, P. minutus, P. crescens and P. costatus, which, however, he does not include in his pedigree. (Fig. 3.)

Hyatt, however, who later examined these fossils,

drew up a very different pedigree, (Fig. 4). He derives all these varieties from P. levis, which he thinks had split up into four varieties before it migrated to the lake in which the fossils were laid down.

Queenstedt and Sandberger, who examined the fossil bed, considered that the faunas of the various layers are not so clearly defined as Hilgendorf makes out, and Hyatt agrees.

Queenstedt went so far as to say that the occupants of these shells had not lived in this hypothetical lake, but the shells, after the death of their occupants, had been carried to the place where they were buried, that the assembly is a composite one.

More recently, however, Gottshick has expressed the opinion that Hilgendorf was right.

This collection of fossils demonstrates the difficulty of determining the genetic connections of the successive fossil assemblies in any locality.

If we accept Hilgendorf's view, the changes undergone by the shells were later reversed, and, therefore, are examples, not of evolution, i.e. the conversion of one type into another, but merely of the reaction of a species to changing environmental conditions. Berg, however ("Nomogenesis" p. 230), accepting Hilgendorf's interpretation, cites this as an example of reversed evolution. I consider this an abuse of terms.

In this connection the reaction of some Scottish red deer imported into New Zealand is of great interest. These deer, thanks to the superabundant food in their new habitat, soon attained a great size and developed magnificent antlers. Later, when owing to their rapid increase in numbers, competion for food became intense, the deer failed to retain their immense size and great

Fig. 3

HILGENDORF'S PEDIGREE OF PLANORBIS

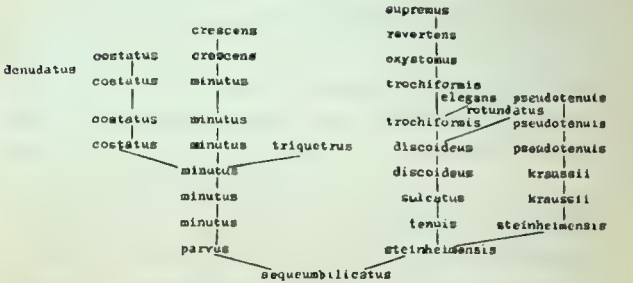
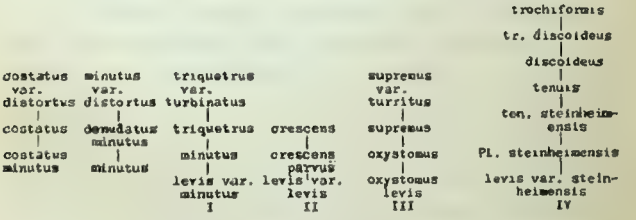


Fig. 4

HYATT'S PEDIGREE OF PLANORBIS



Planorbis levis

antlers, and reverted almost to the Scottish type. This is not a case of evolution and reversed evolution, but of the reaction of an animal to its environment.

Varying environmental conditions account for many, but not all, of the cases of diversity of shell-form displayed by the same species of mollusc. Ingersoll found that of Helisonia trivolvis in small ponds and lakes of Colorado scarcely two specimens are alike and many of them resemble other and altogether different species. (" Darwinism " p. 44.)

It would thus seem that some species are of a composite character, comprising several elementary species.

Nor is this all, similarity of shell form does not necessarily denote close kinship, nor does dissimilarity necessarily exclude close kinship.

Semper, an authority on marine molluscs, writes (" Animal Life " p. 457) : " Zoologists and geologists alike are wont to regard all the land mollusca, or rather their shells, as peculiarly fitted to indicate the affinities and relationship of living and extinct faunas. Now, I do not dispute that they are sometimes of the greatest utility in this respect, but I must here express my conviction—a conviction derived from years of study of the animals as well as of their shells—that in many cases we have absolutely no right to avail ourselves of the shells of the land mollusca for such comparisons; and, moreover, their classification by the shells, which is universally adopted by conchologists and geologists, and which they have accepted as a natural one, is absolutely and totally worthless and unnatural. Thus, every argument based on the assumption that the genera and sub-genera as at present distributed are natural divisions, indicating the true affinity of the species they

include, must be purely imaginary, mere castles in the air (such, for example, as Geotrochus, Bulimus, Rachis, Homorus, Hapalus, Nanina, Leucochroa, etc., etc.; comp. Wallace Geog. Dist. Animals, II p. 512 *et seq.*)"

As specific cases Semper cites (1) the Philippine genus Cochlostyla " of which the shells are so excessively variable—in spite of the similarity of structure in the animals themselves—that no conchologist could possibly describe the genus from the shells." (2) " Hitherto we have always had a genus under the name of Vitrina, but species were included in it which belong, not merely to different genera, but even to different families, e.g. the Zonitidae and the Limacidae."

Haeckel writes (" Systematische Phylogenie ") : "The shells of Molluscs are for the most part of little morphological value and do not permit the drawing of any conclusion regarding the organisation of the soft-bodied creatures that secrete these shells. Snails of very different body build may manufacture altogether similar or hardly distinguishable shells. On the other hand, nearly related snails often construct very different shells; one constructs a very well developed one, another a rudimentary shell, another none at all. This applies equally to mussels and cephalopods."

In the light of the above facts, let us consider the examples of evolution cited by Dr. Davies.

The Family Anomiidae.

Dr. Davies states that some systematists divide up the family of oysters styled the Anomiidae, into two families—the Anomiidae and the Placentidae, the latter

group comprising the window-pane oysters. If this sub-division be accepted and he can show that an Anomia evolved into a Placenta, then he has proved the origin by evolution in nature of a new family. He tries to show that this transformation has been effected. On page 76 he figures a living species of Anomia, a living species of Placenta, two fossil species, Carolia placunoides, from the Eocene of Egypt, and Indoplacuna sindiensis from the Miocene of India, which he deems to be transition forms between Anomia and Placenta. He also mentions that Carolia placunoides, found in Eocene Moqattam beds of Egypt, displays a great deal of variation, some forms being more placuna-like than others. He also cites an undescribed Carolia from the Eocene of India, intermediate in form between Anomia and Placuna. If then he be right, Placenta would seem to have evolved twice over in the Eocene, once in Egypt and once in India. Moreover, from the much earlier bed, the Lower Cretaceous Entaw group of Mississippi and Alabama a fossil Placenta has been described, called Placuna scabra.

The fact that the Carolias in the Moqattam beds displayed so much variation seems to indicate a case of a sedentary form reacting to an unfavourable environment and its shell, in consequence, assuming curious forms. Such reactions among sessile bivalves, are not uncommon (vide p. 187). Moreover the structural gaps between the various members of the series depicted by Dr. Davies are considerable; it is therefore open to doubt whether they are true genetic series.

Fortunately, however, it is possible to put Dr. Davies' theory to an experimental test, because both Anomia and Placenta are living genera. If in nature

the former became transformed into the latter, it ought to be possible, by selecting suitable Anomias, to repeat the process in the laboratory. If this can be done and the Placentas so produced do not yield fertile progeny when crossed with the parent species,geneticists will have performed a great feat, for they will certainly have converted one genus into another, and those systematists who are generally known as " splitters " will assert that a new family has been produced in the laboratory. Until this be accomplished, I think it legitimate to doubt whether the supposed transformation has really been effected.

The Cypraeidae and their Allies

I am indebted to Dr. Davies for calling my attention to the interesting paper on the Cypraeacea (Procs. Malacol. Soc. Vol. XXII. (1936)), in which Dr. Schilder sets forth the results of his careful study of the fossil cowries.

He deems these animals to constitute a stirps, which is equivalent to a sub-order. Having little first-hand knowledge of the Mollusca, I am not qualified to criticise his classification, and I accept it, although his families seem to be rather restricted. It affords support to the thesis that there is no fossil proof of the derivation of any natural family by evolution from an earlier one.

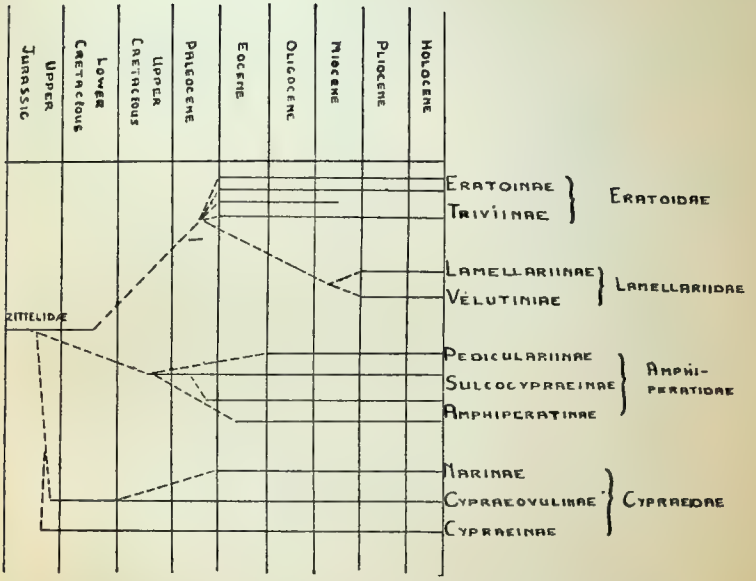


Fig. 5

DISTRIBUTION IN TIME OF THE CYPRAEACEA

(Adapted from Schilder)

Here is his classification:

Sürps Cypraeacea

Super Families	Families	Sub-Families
Lamellarioidea	Zittelidae	Eratoinae
		Triviinae
	Eratoidae	Velutininae
		Lamellariinae
	Lamellariidae	Amphiperatinae
		Sulcocypraeinae
	Amphiperatidae	Pediculariinae
		Narinae
Cypraeoidea	Cypraeidae	Cypraeovulinae
		Cypraeinae

Dr. Schilder has drawn up a pedigree which, in that it shows all the genera, gives more detail than is necessary for our purpose. I therefore produce (Fig. 5) a pedigree in less detail, showing only the families and sub-families. The heavy lines indicate the period during which each family is known to have existed; the broken lines linking the families represent lines of ancestors imagined by Dr. Schilder, but of which no fossils have been found. Thus the heavy lines represent ascertained facts, and the broken lines Dr. Schilder's opinions.

It will be observed that three genera appear in the Upper Jurassic. The first of these is Zittelia, belonging to the extinct family Zittelidae. The second and third represent two subfamilies of another family, the Cypraeidae. Dr. Schilder believes that these originated from the ancestors of the Sulcocypraeinae, which are in turn derived from the Zittelidae, but this is based on surmise, as no fossil of the Sulcocypraeinae is known before the Upper Cretaceous.

The family Amphiperatidae appears in the Creta-

ceous in the form of three genera belonging to the sub-family Sulcocypraeinae, which Dr. Schilder would derive from the Zittelidae. The family Eratoidae appears in the early Eocene as four genera representing both its sub-families. These appear simultaneously in the European and Australian regions. As these Eocene Eratoidae are hardly less distinct morphologically each from the other than are the genera now living, Dr. Schilder believes the family originated in the Upper Cretaceous. The Lamellariidae appear in the Miocene in the form of one genus; this is followed in the Pliocene by a genus representing the other sub-family. Dr. Schilder deems them to be derived from an ancestor common to them and the Eratoidae.

Thus there is no fossil proof that any of the families or sub-families of the Cypraeacea are derived from an earlier family.

It should be noted that there is nothing unusual in the manner in which the various families of the Cypraeacea appear in the rocks known to us, i.e. as two families in the Upper Jurassic, the other three families appearing later, i.e. in the Upper Cretaceous, the Eocene and the Miocene.

Thus, in the case of those Orders of the Pisces which include more than one family, 8 make their appearance in the form of 1 family, 9 in that of 2, 2 in that of 3, 1 in that of 4, while the great Order Beryciformes makes its appearance in the Upper Cretaceous in the form of six families. How are we to interpret this manner of appearance in the rocks known to us?

The evolutionist presumably believes that all these families evolved from a common ancestor in localities of which no rocks laid down during the period of this evolu-

tion have been discovered and, after their evolution, migrated to the areas in which their earliest fossils occur. It is open to the Creationist either to believe of any given family that it migrated from the locality in which it was created to where its known fossils occur, or that it was created in this latter locality.

The Limnaeidae and Valenciennesia

The fossil evidence shows that some Limnaeids and other fresh-water molluscs during the late Miocene and Pliocene in parts of Eastern Europe lived in water that became increasingly brackish.

Many of these molluscs contrived to maintain themselves for a long time in this unfavourable environment, but the beauty of their shells suffered in consequence; first corrugations appeared on these, then, apparently as the result of modifications of the respiratory system, the shells gradually developed siphonal grooves; later, at a comparatively early period of life, the secreting areas of the mantle became affected, with the result that the shell ceased to develop its normal elegant coiling and assumed monstrous proportions, as is shown in Valenciennesia limnaeoidea of which Dr. Davies shows a picture.

Changes of a similar nature seem to have taken place in a number of lineages or species of this genus, and also in the genus Taia. The changes in some of the Limnaeid shells were so considerable that some regard one type, Velutinopsis, as a new sub-genus, and Valenciennesia as a new genus. Dr. Davies would apparently go farther and regard the last as belonging to a new family, despite the fact (see p. 157) that the shell is not a safe criterion on which to base classification.

I have not had the opportunity of examining these fossil Limnaeids, but Gorjanovich-Kramberger's account shows that they assumed so much variety of form that it is not possible with any certainty to make up genetic series out of them. Thus, Valenciennesia exhibits three main types with intermediaries between each; one having strongly-marked pulmonary grooves, another having them feebly-developed, a third showing no trace of a groove.

I gather that Dr. Davies deems Velutinopsis intermediate between Limnaea and Valenciennesia and Velutinopsis pancici to be derived from L. rugosa, and Valenciennesia limnaeoidea from the first; but this does not seem to be the view of Gorjanovich-Kramberger, who drew up a pedigree showing that Limnaea undulata threw off three lines: V. pancici, L. amplecta and V. limnaeoidea. This merely bears out the extent to which these pedigrees are subjective to the feeling of each observer.

This is, however, a detail. There seems little room for doubt that the shells of various molluscs underwent change as a result of the unfavourable conditions to which their owners were exposed, and that these changes resulted in the production of monstrosities, such as those shown by the legs of men suffering from elephantiasis. These monstrous forms were all short lived; all have long ago become extinct, while the Limnaeidae which were not subjected to these bad conditions left descendants resembling themselves, now living in all parts of the world. Had Valenciennesia persisted until to-day, or, any rate, for a long period, before becoming extinct, it might be contended that it was not a monstrosity. Were it now living and had it been

infertile when crossed with Limnaea, then Dr. Davies might fairly have claimed to have adduced fossil evidence of the origin of a family from an earlier one by a process of evolution. As it is he has adduced a beautiful example, not of evolution, but of the effect of unfavourable conditions on the shells of certain Limnaeids.

The Rudists

The Rudists are composed of five families. Of the group as a whole Mr. L. R. C. Cox writes ("The Evolutionary History of the Rudists" Proc. Geol. Assn. (1933) p. 379): "We can only conjecture from what parent stocks the Rudists are derived." The first family to appear in the rocks known to us is the Diceratidae, represented by the genus Diceras in the Upper Jurassic (Corallian). For all the fossils tell us this genus may have been created in the locality in which its earliest fossil occurs. Each of the families, Monopleuridae and Caprinidae, appears simultaneously in Europe and Texas in the latest Jurassic or earliest Cretaceous. The Radiolatidae make their appearance simultaneously in Europe and Texas in the Middle Cretaceous (Albian). The Hippuritidae turn up suddenly in the Upper Cretaceous (Turonian) in various parts of the world: Europe, Sicily, Algiers, Asia Minor, Persia, Jamaica and Guatemala. Mr. Cox hazards no conjecture as to the derivation of the Diceratidae, Caprinidae and Hippuritidae, but he believes that the Monopleuridae are derived from the Diceratidae and the Radiolatidae from the Monopleuridae. Let us examine these views.

Mr. Cox's opinion that the Monopleuridae are derived

from the Diceratidae is based on the fact that the earliest known genus of the former, Valettia, has two well-developed hinge-teeth on the left valve, and one well-developed and one ill-developed (Mr. Cox describes it as "vestigial") tooth on the right valve. This "vestigial" tooth "disappears" in the later Rudists, affording, according to Mr. Cox, "clear evidence that the change in hinge structure is due to atrophy of one of the teeth of the right valve and the development of a tooth in the left valve which had hitherto been rudimentary."

In the majority of the Diceratidae the right valve has two hinge teeth with a socket between them for the insertion of the single tooth of the left valve. This type of dentition is said to be "normal." In some Rudists the left valve had the two teeth and the right the single tooth. This is the "inverse" type of dentition. Mr. Cox believes that, for some unknown reason, a member of the Diceratidae gradually developed a second tooth on the left valve, while one of the teeth on the right valve became degenerate and disappeared, and that Valettia is a species in a transitional condition. I do not believe that this gradual transformation occurred. The Rudists seem to have been endowed with the capacity for developing two teeth in each valve, but, as three functional teeth suffice, only three develop. The potentiality of producing two teeth in each valve was a device enabling the two teeth to be developed in whichever valve these were required, and these developed in the valve by which the animal was attached to a foreign object. In the living Chamidae, whose hinge-arrangement is like that of the Rudists, either the left or the right valve is attached, and the attached

valve invariably exhibits the two teeth. As the Diceratidae were usually attached by the left valve, this had the two teeth, but in the later species which were attached by the right valve, this had the two teeth. Mr. Cox does not name the species of the Diceratidae believed by him to have given rise to the Monopleuridae, and I submit that there is no fossil or other proof of such derivation.

Mr. Cox asserts that the Radiolatidae are "clearly derived from Agria," one of the Monopleuridae. He does not name the species to which he thinks Agria gave birth, nor does he describe Agria, which is not figured in Zittel's Palaeontology. Dr. W. H. Dall, who wrote the account of the Rudists in that textbook, considers that the Monopleuridae, supposed by Mr. Cox to have given birth to the Radiolatidae, are rather far removed from the latter, as he places them in different superfamilies. He classes the former among the Chamaceae and the latter among the Rudistaceae.

Thus Mr. Cox's view as to the origin of the Radiolatidae is conjectural.

I submit that the known Rudist fossils afford no evidence of the origin of a new family by a process of evolution.

The Nassidae or Dog-Whelk Family

Dr. Davies cites the Nassidae as evidence, not of the evolution of a family from a pre-existing one, but of our imperfect knowledge of the Palaeontological record.

The Nassidae, however, deserve mention in connection with the supposed evolutionary origin of new families, because they illustrate the proposition: if the evolution

theory be true, then all major evolution, as opposed to mere differentiation, has taken place in localities which have not been geologically explored.

According to Dr. Davies the earliest known fossil of this family is of the genus Brachysphingus of the sub-family Dorsaninae, which makes its appearance in the Palaeocene in California and possibly in the Paris basin, followed shortly after by the genus Molopophorus These genera exhibit all the distinctive characters of the Nassidae, and Dr. Davies does not suggest any earlier genus or family from which they evolved. In the middle Eocene a species of the sub-family Coptaxinae makes its first appearance in the Paris basin. In the upper Eocene the third sub-family, the Nassinae,makes its appearance "approximately at the same time in Java and Peru." Later this sub-family became widespread and exhibited great variety.

Each of these sub-families appears to have originated in the sea at some distance from land, and, as conditions became favourable in the coastal seas, to have migrated to the localities in which the earliest known fossils have been found. The evolutionist supposes that all these sub-families are descended from a common ancestor, which is itself derived from the common ancestor of all the Metazoa.

The scientific creationist is far more cautious. He points out that the Nassidae have undergone very little, if any modification since they first appeared, that the genera Dorsanum, Nassa and Bullia have all persisted since the Eocene. This being so, there is no valid reason for supposing, in default of fossil evidence, that these genera underwent great transformations before their first appearance.

Until further fossil evidence be found the Creationist declines to express an opinion as to whether the family, or each sub-family or even each genus was separately created. I leave it to the reader to judge which attitude is the more scientific.

The above, then, are the attempts made by Dr. Davies to adduce fossil proof of the origin of a natural family from an earlier one. I maintain that in none of the cases is the attempt successful.

CHAPTER XVIII.

The Nuculidae

[Dr. Davies's most interesting account of the Nuculidae contains much that is new to me. It illustrates the unsatisfactory state of zoological classification and nomenclature. As the various categories family genus, species and the like mean one thing to one zoologist and another to another, it is clearly impossible for a Creationist, no matter what his particular views are, to express in scientific terms what the units of creation are.

Under the mistaken impression that I assert the zoological family to be the unit of creation, Dr. Davies charges me with having proved too much in showing that, no matter how far we trace them back, Nucula and Acila show no signs of merging into one another, unless I regard them as belonging to different families.

He puts to me a question which I as a creationist, did not expect, viz: What kind of blending or transition between these two forms I would expect? The answer is that I do not *expect* to find such blending.

If each family be a special creation, I see no reason why it should not be created as a group of genera or species.

We have to learn much before we can hope to elucidate the question of the units of creation. The first step in attempting this is for a number of zoologists

to shake off the incubus of transformism, and to realise that it is in the highest degree improbable that all living organisms are descended from a common ancestor.

As regards Acila isthmica, I cannot see any insuperable physiological or mechanical reason why its teeth may not be derived from the more usual short teeth, either as a mutation or gradually. I do not say that this has been effected. I do not know. As regards the date of the first appearance of Nucula. When looking for examples of genera that have persisted over long periods I consulted Zittel's Palaeontology. It now appears that Nucula has been subjected to treatment meted out to many genera of late years.

I speak feelingly on this matter, because, as an ornithologist, I am exasperated by the frequent changes that are made in the scientific names of birds.

Dr. Davies says that the first records of fossils are often made by geologists with only a very general palaeontological training and these naturally refer them, if they possibly can, to some known genus. Doubtless some fossils have been thus wrongly classified; but subsequent investigators appear to have erred in the opposite direction, with the result that new genera have been instituted on inadequate grounds, or, perhaps I should say, in order to bring the animals into line with evolutionary concepts. Persistent types in a supposed world of perpetual flux demand explanation, not always easy to find; hence the desire that these types should be few and far between.

This assigning of new names to genera has roused many protests from men of science. Professor McCready Price quotes the following extract from a

letter of "a geologist...whose work on the Palaeozoic fossils is recognised on both sides of the Atlantic," ("Evolutionary Geology," p. 187): "Some geologists make it a point to give a new name to all forms in Palaeozoic rocks; that is a name different from those of modern species. I was taken to task by a noted Palaeontologist for finding a Pupa (a kind of land snail) in Devonian beds; but in which I could not find any point in which it differed from the modern genus."

My friend Mr. G. K. Hebbert, whose special subject is Lepidoptera, writes in a private letter "Every one appears to re-classify the fossil insects according to their own theories about evolution," and again "The rule for insects seems to be that anything older than Oligocene you are free to place in any Order that may suit your private theories."

These complaints are not all confined to private letters. Heilprin writes ("Distribution of Animals" p. 183): "However divergent be the views of authors on the matter of relationship, it is practically certain that numerous forms of life, exhibiting no distinctive characters of their own, are constituted into distinct species for no other reason than that they occur in formations widely separated from those holding their nearest of kin."

In the same work Heilprin gives (p. 282) an account of the way in which insects originally deemed to be Devonian were hoisted up into the Carboniferous and entirely reclassified. Hagen placed these ancient insects in the modern groups: Libellulae and Neuroptera. But Goldenbeg, Brogniart and Scudder, asserting that they were all synthetic types, constituted for them a new order, the Palaeodictyoptera; these insects are

now not true Neuroptera, Orthoptera and Hemiptera, but neuropterid, orthopterid and hemipterid Palaeodictyoptera! This procedure was justified by the assertion that all these are more closely related to one another than "any one of them is to that modern group to which it is the most allied and of which it was with little doubt the precursor or ancestral type (Scudder)."

Heilprin asks "Surely it will not be contended that Palephemera and the highly specialised Titanophasma are more nearly related to each other than they are to the modern families, Libellulidae and Phasmidae, not to mention the orders to which these belong; and if this be so, why should they be referred to the one loose comprehensive group rather than to the several groups which they immediately represent?" Handlirsch, more recently, has accepted Scudder's view. I am not competent to decide which view is correct, but there can be no doubt that the view of Scudder and Handlirsch is the one that coincides with the evolutionary concept. I wish to emphasise the way in which nomenclature and classification are to-day dominated by philosophical considerations.

This is equally true of the periods to which fossiliferous rocks are assigned. Some of these early insect fossils occur in the Little River Group of St. John, New Brunswick. This formation, we read (Z. vol. 1. p. 820) "was formerly regarded as of Devonian age, but is now assigned on the evidence of Palaeobotany to the lower Productive Coal Measures (Carboniferous)." This is because the plant fossils in it are like those which occur elsewhere in Carboniferous rocks. But the geological evidence points very strongly to the formation being of Upper Silurian Age. Dr. L. W. Bailey in his Presi-

dential address to the Geological Section of the Royal Society of Canada in 1919, said: "It is to be noted that the view they are Carboniferous rests solely on palaeobotanical grounds, while no one who has actually studied the ground has yet reached any other conclusion than that they are really older than the formation last named." (See p. 133) But so far as I am aware, all who have written since 1919 treat the fossils in these rocks as carboniferous; to do otherwise would raise many difficulties for the evolution theory.

We must now return to the renaming of fossils by evolutionists. Scores of examples could be cited. Here are a few.

Heilprin (*Op. Cit.* p. 138) states that the living mollusc genus, Nautilus has persisted almost unaltered from the Silurian period until to-day. Barrande laid great stress upon the sudden appearance, in the full plenititude of their power and side by side, of the distinctive genera of the cephalopods (Orthoceras, Cytoceras, Bathmoceras, Nautilus) in the Lower Silurian. Then Hyatt came along and decreed "there are no true species of Nautilus in Palaeozoic rocks." (Proc. Boston Soc. Nat. Hist. XXII, p. 253 (1883).)

In consequence all the species of Nautilus found in Palaeozoic rocks had their names changed, some are now assigned to the genus Plectoceras, others to Litoceras, and yet others to Endolobus.

Davidson writes in his review of the British fossil Brachiopods ("Palaeon. Soc. Rep. 1884): "the resemblance between the recent Rhynchonella nigricans and some Cretaceous and Jurassic forms is so great that we are at a loss to define their differences." Dr. Davies tells us ("An Introduction to Palaeontology" p. 36)

" the Palaeozoic and Triassic forms have been separated off into separate genera, the sorting of the Jurassic and Cretaceous species is only begun."

In the Palaeozoic rocks a number of fossils have been found which used to be described as those of pteropods by all palaeontologists. Thus Dana writes in the 4th edition of his Manual of Geology, (1896) in which some of these fossils are figured: " Other eminently characteristic (Cambrian) molluscs are the pteropods of the genera Hyolithes and Hyolithellus." The genus Hyolithes is known to have persisted until the Devonian Period. Dr. Davies writes (" Introduction to Palaeontology " (1920) p. 121) : " among the fossils from the Middle Cambrian of British Columbia, Mr. Walcott has found a Hyolithes showing swimming organs closely resembling those of modern pteropods," and we " find in the Devonian rocks abundant shells indistinguishable from those of the modern pteropod Styliola."

But pteropods are what evolutionists describe as "very advanced " types of molluscs, hence their presence in the earliest known fossiliferous rocks is not in accordance with the doctrine of evolution; moreover, some evolutionists, including Dr. Davies, are convinced that the pteropods are derived from the supposedly more primitive opisthobranchs, which are not known earlier than the Carboniferous Period. Obviously the evolutionists have to adopt some device to bring the above facts into line with the concept of evolution. Broili has offered a solution of the problem, and Dr. Davies is among those who have accepted the solution, which is the simple device relegating the Palaeozoic pteropods, bag and baggage, to another order or class— the Conularida. Mr. G. C. Robson writes (B. vol. 10,

p. 69) : " It is better to accept Broili's verdict. Nevertheless, if these remains are subsequently proved to be those of pteropods, and if the hiatus in time between their appearance and that of the other Opisthobranchia is not merely due to the imperfection of the geological data, then we shall be driven to one of the two very interesting conclusions. It will be necessary to assume either that the Thecosomata (i.e. pteropods) were developed directly from the primitive streptoneuran stock and are not from the Opisthobranchia, as is usually believed, or that the Cambrian Thecosomata have nothing to do with modern ' Pteropoda,' but represent an early essay in pteropod-like specialisation."

Doubtless one of the reasons for the popularity of the evolution theory among biologists, is that it offers great scope for one's ingenuity in explaining away facts that do not readily fit the theory.

When a persistent type baffles the efforts of systematists to change its name, the evolutionist may make it conform to the transformist doctrine, by asserting that the type in question has evolved on more than one occasion !

Mr. F. Chapman discovered in the Cambrian deposits of Malvern a fossil Foraminifer, Spirillina groomii, and Messrs. Heron-Allen and A. Earland found this creature alive off the West of Ireland. As the idea of a species existing unchanged from Cambrian times onwards is difficult to reconcile with the transformist concept, L. Rhumbler considers that, not only Spirillina but also Ammodiscus, and Girvanella found from the Silurian upwards, have arisen several times. L. Berg thinks that the cirripede crustacean Pyrgoma which occurs in the Devonian and Tertiary deposits has

evolved twice over. If the same species can be evolved from different ancestors, there can be no certainty that any two very similar animals are nearly related.

In my view it is unfortunate that the great "Fossilium Catalogus," now under preparation is being compiled entirely by adherents of the doctrine of evolution. In the present state of affairs this seems almost unavoidable, because, apart from Lt. Col. L. Merson Davies, who specialises on Foraminifera, living palaeontologists all seem to accept this doctrine. This is an example of the way a Science suffers when it adopts a creed. If every fossil could be scrutinised by experts, some of whom accept transformism and others do not, the value of the catalogue would be enhanced.*

* While this book was in the press I learned from the newly-published Tome V. of the "Encyclopédie Francaise" that Prof. P. Lemoine, a past President of the Geological Society of France and Director of the Muséum d'Histoire, (whom I have quoted at the beginning of this volume) and apparently other French naturalists have discarded or are discarding the theory of evolution. Thus the biological outlook is brightening.

CHAPTER XIX.

"Evolution" within the Family.

Let us now notice the cases adduced by Dr. Davies of evolution within the family. I submit that this expression reveals a defect in biological terminology. It is unfortunate that the word used to describe the supposed gradual transformation of an amoeba into an elephant is also employed to describe the insignificant modifications which animals undergo when subjected to new conditions of life. It would make for clarity and precision if these minor changes were described as differentiation rather than as evolution.

This would not prevent the evolutionist from trying to show that differentiation is the first stage of evolution.

Modifications of the Viviparids in the Levant during the Pliocene.

We have noticed how readily the forms of the shells of molluscs react to environmental conditions. (p. 157).

The fossils show that Viviparids were abundant in the Levant during the Pontian, Cimmerian, Rumanian and Slavonian periods. In the earliest of these their shells were smooth and rounded; they became first more expanded, then keeled, and eventually the keels exhibited tubercles. There can be little doubt that these modifications were the consequences of environ-

mental changes, although there may have been replacements owing to migration. The modifications in question seem to have been mere somatic reaction to an unfavourable environment and not evolution, because (1) the original smooth type of Viviparid still exists, while the "evolved" types are mostly extinct, (2) other molluscs in the locality became modified in a similar manner; some of these were different genera and even of a different family. Neumayr records (N. p. 283) that in the genera Melanopsis, Bythinia, Neritina, and the bivalve, Unio, the shells became thicker and developed ribs and nodules. He attributes these changes to the freshening of the water. (3) The extinct genus, Tulotoma, seems to have undergone similar modifications, as does the viviparid, Margarya, in a drying lake in Yunnan.

Bateson showed (Phil. Trans. R. S. 1889, p. 297) that, as the lakes near the Aral Sea dried up and so became salter, the shells of the cockle (Cardium edule) living in them became progressively smaller and thinner, and on the inner side of the shell grooves developed between the ribs. Dall records that as the lakes in Utah, Nevada and California dried up, the sculpture of the shells of all the Molluscs in them became more complicated.

It is improbable that the change in the salinity of the water was the direct cause of these shell modifications. Bateson expressed the opinion that nutrition had something to do with them. In each case the modification was evidently a reaction to unfavourable conditions.

Dr. Davies quotes (E. p. 127) Annandale's statement (Proc. R. S. Ser. B. vol. 96. 1924): "In certain regions of the earth's surface there is or has been some influence

at work which has produced a similar collective peculiarity in the shells of the Viviparidae on diverse occasions and in different parts of the world. In many countries there is no evidence that anything of the kind occurred." Annandale suggests it may be due to some chemical stimulus. He expresses the belief that these environmental influences, if exerted for a sufficiently long time, affect the germ plasm and thus new species may arise.

In this connection he makes some interesting observations on the allied genus Taia. It is possible to arrange the shells of individuals of this genus living in Burma in ascending order according to the roughness of the shell and thus form a series the first three members of which, found in Upper Salween, Annandale deems to be varieties of the species T. naticoides; the last three of the series, taken from various parts of Lake Inle, he regards as separate species: T. shannensis, T. littoralis and T. intha. T. naticoides, the smoothest-shelled species, usually found in swamps, backwaters and canals, is very variable; the three types of its shell, viz. almost smooth, having simple ridges, having nodular ones, constitute the first three of his series of six. He deems these mere varieties, because shells in the same spot and in precisely the same environment show considerable individual variations in sculpture. It should be noted that the few fossil shells of Taia found in an old lake bed have highly sculptured shells; therefore, the evolutionist has to regard the T. naticoides now living as more primitive, less evolved than the fossil ones. Annandale considers the last three of his series to be, not varieties of naticoides, but different species, because they differ in the form of their radulae

and gill filaments and in the structure of their central nervous system, also in the number of complete embryos they produce. Naticoides may have more than 30 embryos, but one race usually has six: shannensis has, as a rule 5, elitoralis has usually three, sometimes 5, and intha only one. It is submitted that these differences of themselves are insufficient to entitle the races to specific rank. The difference in the number of embryos certainly does not; one race of naticoides produces five times as many as another. Again, the shrimp, Palaemonetes varians, has two varieties or races: P. v. microgenitor, which lives in the sea and produces about 320 eggs in the year, and P. v. macrogenitor, which lives in fresh water and lays only 25 eggs; the eggs of the latter are three times the size of those of the former. The differences between shannensis, elitoralis and intha appear to be modifications resulting from different environmental conditions. Shannensis, which has the least rough shell, lives among the floating islands that form a ring round the lake; elitoralis, which has a rougher shell, occurs on the outer margin of these islands, while intha having the roughest shell, inhabits the clear waters in the central part of the lake.

Of these four types of shell, naticoides is the most variable, shannensis less variable, elitoralis more constant and intha least variable. It may be noted that fossils of naticoides have been found in two recent beds in the same locality one of peat and the other of clay; the fossils in the two beds differ slightly from one another.

Annandale agrees that the differences between these four types are due to environmental conditions, he thinks that favourable enviroment induces sculptur-

ing. The most sculptured form, intha, lives in remarkably clear water of which "the temperature apparently varies little and it is not as a rule subject to violent agitation through storms. There is a fairly abundant growth of algae on which Taia feeds, but this growth is not apparently inimical to animal life as too great a prolification of freshwater algae frequently is. T. intha has no enemies, so far as I could discover, except a leech.....which is often present in its branchial chamber. It has practically no competitors for food... T. shannensis lives in very different conditions. The water is dark and much contaminated with rotting vegetation...There is abundant shelter, but, on the other hand, wading birds which might spread cercarial infection among the molluscs, as well as feeding on them, are common, and also predaceous fish...competitors for food...are plentiful, but the conditions are on the whole more stable than in a mere swamp. In the intermediate zone inhabitated by T. elitoralis conditions are intermediate."

My view is the fact that intha has so few competitors indicates an inclement habitat, in which it contrives to exist, and this has resulted in its highly sculptured shell and the small number of eggs it produces. But this is a detail. The matter of importance is that the modifications of these Taias appear to be a direct effect of the environment. In default of evidence as to whether these forms will interbreed if afforded the opportunity, we cannot be sure if they are all of one species or not.

In this connection two other facts have to be considered. The first is: near the Inle Lake "there are two localities at which large deposits of Taia shells have been found. One is in a couple of limestone caves

...into which...shells have been washed through holes in the roof. Here three species of Taia (T. obesa, T. conica, and T. cylindrica) were found, different but quite analogous to the three Inle species. No information was obtained as to their original provenance, and in the circumstances it is not surprising that they were all mixed together. Recent examination of much larger series than I had before me in describing the shells confirms my belief that they are quite distinct specifically. (Annandale.)" The last statement is rather surprising in view of the great variability of the shells of many molluscs, and those of Taia in particular.

The other fact is that Vivipara having ridged shells are now living in various localities, e.g., Assam, Burma, Malay Islands, Celebes and the Philippines.

It is submitted that these Viviparids afford an illustration, not of evolution but of the manner in which the appearance of the shell varies with environmental conditions. Whether Annandale is right in thinking that peculiar environmental conditions, if they persist sufficiently long may eventually change the nature of the germ plasm of organisms subjected them has yet to be demonstrated. We know that external conditions affect the form of individuals, but there is no proof in any case that the changes so brought about have persisted long after the causes effecting them have ceased to operate. Nor is there any proof, or even evidence, that external conditions can so affect the germ plasm of individuals that these are sterile when crossed with individuals of their species which have not been subjected to such external conditions. The results of genetical work indicate that the fundamental character of the gametes, or reproductive cells, is affected little, if

at all, by environmental conditions.

Gryphaea, Exogyra and other Oysters

Dr. Davies cites the extinct oyster Gryphaea, known as the devil's toe-nail, as an example of "evolution" within the family. He describes (E. p. 130) the modifications whereby the comparatively flat Ostrea irregularis seems to have become transformed into the much curved Gryphaea arcuata, and gives pictures illustrating this. He writes: "It seems possible that these changes are adaptations to increased muddiness in the water. They are repeated time after time in different stocks during the Jurassic period. The end-forms of each lineage seem to have become extinct, but their striking features, differing so much from those of ordinary oysters, has led to their being united as a separate genus Gryphaea. This is a good example of a 'polyphyletic genus,' due to parallel development."

I maintain that this is a case, not of evolution, but of abnormal growth of the shell resulting from the environment becoming increasingly unfavourable.

Dr. Davies makes no mention of other types of abnormal development of the shell, such as the Exogyra type in which the curved beak is twisted to one side.

I take the liberty of quoting the following passage from Professor Neaverson's invaluable "Stratigraphical Palaeontology" (p. 146): "It must be remembered that while such forms as oysters are very common in nearly all Mesozoic deposits, their investigation is beset with great difficulties, owing to the extraordinary variation in the form of the shell. As Woods says in his study of the Cretaceous Lamellibranchs: 'The

variation has been brought about by changes in the physical condition of the habitat, and particularly by differences in the character of the surface to which the left valve is fixed; it is found that the mode of growth and ultimate shape of the shell are determined mainly by the size, shape and position of the attached surface, so that commonly any one species shows an amazing variety of forms which can, however, be linked together by a large series of specimens.' The ostreid group has been subdivided into numerous 'genera,' of which Ostrea, Alectryonia, Exogyra and Gryphaea are most commonly cited. The characters on which these series are based have probably arisen several times during the history of the group. Exogyriform oysters are marked by a tendency for the shell to bend laterally and thus to show a twisting of the umbonal region. Gryphaeate oysters tend to thicken the left (attached) valve and reduce the area of attachment. Some forms usually classed as Alectryonia, have radial folds, whose presence gives similarity of appearance but does not necessarily indicate close relationship."

In some cases the abnormality became so great that the shell could not open properly; this of course meant extinction. Dr. W. D. Lang has suggested that these abnormal forms became extinct through secretion of a superfluity of calcium carbonate. This may have been the case and the cause of this excessive secretion been exposure of the oyster to unfavourable conditions. It would seem that these abnormal types of oysters are comparable to the monstrous shapes assumed by goldfish reared in insufficiently oxygenated water. (see p. 198). They afford examples, not of evolution but, of the pernicious effects of unsuitable environment.

In this connection it is important to bear in mind, first, that the parent type Ostrea has persisted and flourishes to-day while the "evolved" types became extinct long ago. Secondly that the Ostrea type of shell is capable of assuming a great variety of shapes; considerably more than fifty extinct species are known and the specific names of these indicate the divers forms assumed: acuminata, biauriculata, carinata, compressirostra, deltoidea, falcata, quadruplicata, sellaeformis, etc. Thirdly, that Ostrea, Exogyra and Gryphaea may occur in the same formation, indicating that all types did not react in the same way to environmental conditions.

Inoceramus

The extinct bivalve Inoceramus is cited by Dr. Davies as an example of "evolution" within the family. Professor H. Woods' paper (Q. Jour. Geo. Soc. (1912) vol. 68. pp. 1 to 20) on this subject is very interesting. The history of this genus, so far as it is known, is like that of many others that have become extinct; in the greater part of the period during which it is known to have existed, it is very stable, then, shortly before its extinction, while it is, so to speak, in its death throes, it assumes various strange forms. It is thus described by Professor Woods: "Although the genus Inoceramus is found in deposits as early as the Lias, yet it is represented by comparatively few species until the close of the Lower Cretaceous Period, after which it underwent rapid evolution so that many species and varieties were developed; but, so far as we know, none of these survived the Cretaceous Period or left descendants." In the above

passage I would substitute for "after which it underwent rapid evolution so that many species and varieties were developed" the following: "after which its environment became increasingly unfavourable, in consequence its shell assumed many strange forms."

In the course of these changes, the valves, particularly the left, assumed abnormal shapes and departed more and more from the flattened oyster type, and this necessitated changes in the hinge.

Professor Woods is of opinion that the forms in which the valves, while retaining their original shape, developed radial folds, e.g. etheridgei, tenuis, concentricus, subsulcatus and sulcatus are derived from Inoceramus salomoni, of the Lower Greensand, while those of which the left valve increased in size, curled over the right valve and, in some cases, developed radial folds, are derived from I. necomiensis. He has drawn up a pedigree according to his views, which he admits is to some extent guesswork. Assuming that his pedigree is correct; this, I submit, is not a case of evolution, but of reaction of various lineages to environmental conditions which became progressively unfavourable and eventually unendurable, so that the genus died out. All the later forms are abnormalities. We must bear in mind that organisms attached to fixed foreign bodies are completely at the mercy of the conditions prevailing at the spot where they are fixed. As in the case of the Viviparids and of Exogyra and Gryphaea, so, in that of Inoceramus, similar changes occur in a number of different species. Professor Woods remarks: "It is interesting to note that radial folds have developed independently in several groups of Inoceramus;" he names five of these. The genus in North America

underwent changes similar to those of the British species.

Volutocorbis and Volutospina

Since 1909, when Dr. Burnett Smith wrote his paper entitled "Phylogeny of Volutilithes," this genus has been split up, in my opinion unjustifiably, into the two genera, Volutocorbis and Volutospina.

A glance at Plate III in Dr. Davies' book purporting to illustrate the "evolution" of the Volutidae in the Eocene seems to indicate very clearly modification resulting from unfavourable environment. It was therefore with considerable interest I consulted the above paper (Proc. Acad. Sci. Philadelphia, vol. 58), in order to ascertain the views of the author. I find that these largely coincide with mine. He writes (*op. cit.* p. 73) it is evident that the deposits in which the shells under consideration occur "were formed in a great shallow arm of the sea whose waters were sometimes fresh or brackish and at others salt"—an environment not favourable to marine animals. "The marine faunas" he writes, "which from time to time invaded this Lignitic Gulf brought with them, at first species and races of Volutilithes with normal aspect. Those forms which were subjected to the conditions of the Lignitic eventually followed a course of evolution which was a direct reflection of their unfavourable environment. The races of V. petrosus at Bell's Landing, Wood's Bluff and Hatchetigbee Bluff make such a series in which the senility becomes more and more extreme with the course of time. Occasionally throughout the later lignitic, as at Yellow Bluff, we find a race which has

migrated from a more favourable environment; which resembles the primitive races of earlier lignitic waters... The grade of phylogenetic development which the Yellow Bluff assemblage has attained also indicates that a normal slow and even evolution has been going on in one locality, while at the same time, rapid senile evolution has taken place among those subjected to unfavourable conditions in another region."

Had I written the above passage, I would have substituted " degeneration " for " evolution " and " less unfavourable " for " more favourable," because that the comparatively shallow sea outside the Lignitic was not really favourable and the Volutilithes living in it were slowly degenerating is rendered clear by the fact that one of the two species still living, viz. V. abyssicola, found in relatively deep water near the Cape of Good Hope is, to quote Burnett Smith, " quite close in its grade of evolutional development to the ancestral V. limopsis of remote Eocene time." This would not have been the case if evolution be a normal state of affairs; the species would not have remained unchanged during the whole of the Tertiary Period. Nor is this all: Volutilithes displaying a callus, like that on the specimen figured by Dr. Davies, occur in the Miocene of Europe. Dr. Burnett Smith deems this a case of parallelism; I submit that it is rather one of similar reaction to unfavourable conditions.

Syringothyris

Dr. Davies writes (E. p. 129): " The brachiopods of the Carboniferous Limestone also show evolutionary series, of which one—that of Syringothyris—is illustrat-

ed in Plate IV., upper figure." If this series be evolutionary, the evolution took a curious course, because the middle term is not intermediate in form between the upper and lower. Dr. Davies does not name the species depicted, but the bottom one appears to be S. principalis, the middle one S. cuspidata mut. cuspidata, and the top one S. elongata, all of which are figured by Dr. F. J. North on Plates XI and XII of vol. 76 of the Quarterly Journal of the Geological Society. But he regards these three not as parent, child and grandchild, but as parent (principalis) and two children. This, however, is a trifle.

As the mode of life of brachiopods is similar to that of sessile molluscs, it might be supposed that their shells are very variable. This is the case. Thomas writes of the fossil Producti (Mem. Geolog. Survey, Palaeontology, vol. 1. pt. 4. p. 253) : " It is becoming more and more recognised that possession of similar features alone may be misleading and an insufficient criterion for the estimation of species. Externally the individuals may be morphologically almost indistinguishable and yet belong to totally different lines of development and therefore to different genera." Dr. North states (*op. cit.* p. 212) that this is equally applicable to the Spiriferids to which Syringothyris belongs.

This genus affords an excellent illustration of the changes undergone by the shell of an organism exposed to increasingly difficult environmental conditions before it dies out. Dr. North writes (*loc. cit.* p. 181) : " The genus illustrates in an interesting way some of the principles of evolution. The (phylogenetically) young stage with small and simple forms is represented by the earlier mutations of cuspidata and elongata : the adult

stage in which the full size is attained and all the characters are mature, is seen in the mut. exoleta and, in the Canina Zone, in representatives of S. elongata: while phylogenetic old age with 'exaggeration and per-elaboration,' followed quickly by extinction, was reached (in normal limestone deposits) in the Dibunophyllum Zone, but was, in certain areas, hastened by the special conditions accompanying the formation of 'reef-knolls.' "

What the fossils here show is two lineages of the brachiopod Syringothyris principalis living first in fairly favourable conditions, then in very favourable ones; after which the conditions become increasingly unfavourable and finally unendurable. The shells reflect these changes. The varieties of the shells from start to finish do not appear to be greater than those exhibited by the living dog-whelk, Purpura lapillus. I submit that it is a misnomer to call such modification evolution.

ZAPHRENTIS

The horse will have to look to his laurels and produce a few more fossil links if he is not to be ousted from his proud position as chief witness on behalf of Evolution. Of recent years his position has been seriously challenged by the Carboniferous coral, Zaphrentis, and the Cretaceous sea-urchin, Micraster. These two have recently been selected as affording the best testimony on behalf of evolution by such authorities as Professor D. M. S. Watson ("Palaeontology," B. vol. 17, pp. 108-110,) Dr. Bather (Proc. S. W. Nat. Union (1931) p. 35,) and Dr. Davies. Let us now examine

Zaphrentis. As Prof. Watson in the article cited above gives a picture illustrating the "evolution" of Zaphrentis, the general reader has some opportunity of forming an opinion regarding this.

I regret to have to decry Zaphrentis, and to say that, through no fault of his own, he is not a satisfactory witness. In the first place he is an extinct coral, hence his testimony is confined to his hard parts, that is to say his skeleton which is outside his body and secreted by this. Thus, like the shell of the sessile mollusc, the hard parts of the coral are very variable owing to being so much at the mercy of the condition of the surrounding water.

The second defect of Zaphrentis as a witness is that it is a genus that exhibited a great diversity of form, and several varieties or "species" are sometimes found in the same locality. Curiously enough in the issue of the Quarterly Journal of the Geological Society (1910) in which appears Mr. R. G. Carruthers' paper describing the evolution of Zaphrentis there occurs immediately following it a paper by Mr. A. Wilmore on the Carboniferous limestones of part of Yorkshire, in which he records his discovery in the deposits of one quarry of fossils of no fewer than five species of Zaphrentis, all different from those described by Mr. Carruthers, and one of these species, Zaphrentis amplexoides, exhibits a number of features associated with different genera.

With these preliminary remarks in mind let us see what the 1200 Zaphrentis fossils from Scotland, examined by Mr. Curruthers, tell us. The changes which he believes to have taken place are so small that I find no difficulty in believing that they may have taken place

in genetical series. They consist chiefly of the shortening and thinning of the septa and of changes in the shape of the cardinal fossula, which is a groove in the floor of the chamber in which the coral polyp lived.

The Zaphrentis fossils examined by Carruthers occur in the following beds, arranged in descending order, no. 5 being the latest to be deposited:

5. Millstone Grit
4. Upper Limestone Group
3. Lower Limestone Group
2. Lawston Limestone
1. Cementstone Group

A long interval elapsed between the deposition of the Lower and the Upper Limestone, during which the sea receded and seams of coal composed of land plants were laid down. This freshwater formation contains no fossils of Zaphrentis, as the latter inhabited the sea. Thus, there is no continuity between the marine fauna of the Lower Limestone and that of the Upper. The fossils in the latter were all immigrants from the sea.

Mr. Carruthers' supposed evolutionary series begins with Zaphrentis delanouei, and ends with Z. disjuncta with Z. parallela and Z. constricta coming in between. It should be noted that the four members of this series are not very sharply defined; there is a little overlapping.

Of 1200 fossils collected by him not a single specimen of disjuncta and only one of constricta was found in the earliest formation—the Cementstone group, but 74 delanouei and 33 parallela were obtained from it. In the next deposit, the Lawston Limestones, all the 55 fossils found were of another species of Zaphrentis—Z. lawstonensis—a form that occurs in none of the other

deposits dealt with by Mr. Carruthers. In the Lower Limestone group he obtained only 2 fossils of delanouei, and 22 of parallela, as against 474 of constricta and 168 of disjuncta. In the Upper Limestone no fossil of delanouei and only 1 of parellela was found, as opposed to 40 of constricta and 271 of disjuncta.

Mr. Carruthers interprets the above facts as: delanouei evolved into parallela, the latter into constricta, and constricta into disjuncta. He supposes that some individuals evolved faster than others.

The facts are open to a very different interpretation, viz. that the above four "species" are not genetically connected, that the conditions of the locality were at first favourable to delanouei and parallela, and then became gradually unfavourable to them, but favourable to constricta and disjuncta, in consequence the latter gradually replaced the former.

Points in favour of Carruthers' view are (1) the various forms sometimes overlap, (2) the great variability of the genus.

Points in favour of the other view are (1) in every locality at least two of his species occur together, while in three different localities, viz. East Barns Quarry, Duloch Quarry and North Mine Quarry all four occur, (2) all his four species are replaced by a fifth in the Lawston Limestone, (3) in the Upper Limestone Group the percentage of disjuncta rises considerably as we pass from East to West. Carruthers suggests three possible ways of meeting this difficulty to his theory: (a) The western limestones may be of slightly later date than the eastern. (b) The assumption of the disjuncta habit may have arisen, not so much from an inherent cause, as from some change in the physical

environment, for instance a shifting ocean-current advancing slowly from west to east. (c) The forms defined as Zaphrentis disjuncta may in reality belong to two branches, that prevalent in the west diverging from the original constricta stock more rapidly than the eastern form. (4) Fossils of constricta have been found in Westmoreland and probably Derbyshire, and those of disjuncta in Cheshire.

From the foregoing it is apparent that it has not been proved that the later members of Carruthers series are derived from the earlier ones.

Moreover, we have no means of knowing whether or not the later forms of Zaphrentis would have yielded offspring had they been mated with earlier forms.

Micraster

Micraster is a better witness than Zaphrentis because the changes it exhibits are those of its organs and not merely of its habitation, and it is not so prolific in species as Zaphrentis.

Dr. Rowe collected some 2000 fossils of this sea-urchin from the Middle and Upper Chalk of England and recorded the horizon at which each was found. He then took the dimensions of each specimen and noted its peculiarities. He thus discovered that 18 characters could be recognised in the test or integument, each of which changed in a definite direction in course of time.

Thus the circular mouth gradually assumed a crescentic shape, a lip grew over it, the grooves in the anterior petal became more and more pronounced, the smooth surface between the pores became granular, and so on. Although these changes were observed in all

specimens they were effected earlier in some than in others. As there is every grade of transition between the earliest and the latest conditions of each of the characters, there can be little doubt that the changes occurred; but it is probable that there existed several lineages of Micraster, each of which changed at a different rate. Systematists have described the earliest of these Micrasters as Micraster cor-bovis, those in the middle period as M. prae-cursor and the latest as M. cor-anguineus, but it is impossible to draw definite lines between the three types.

Professor MacBride is of opinion that the above changes were adaptions to the sea-urchin's burrowing deeper in the sea floor as time went on. Thus, it became necessary for the creature to hold the mud away from the gills by developing the carina or crest carrying a fan of curved spines. I am inclined to agree, and so apparently is Dr. Davies. The sea floor seems to have become progressively softer, causing the sea-urchins to sink deeper into it.

A species that persists for a long period is likely to encounter changes in environmental conditions from time to time, just as a widely-spread species does from place to place at any given period. Even as such differences in the case of the latter result in the development of geographical varieties or races, so in the former they produce what we may term temporal races or varieties. This is what we find in Micraster; cor-bovis may be regarded as an early race and cor-anguineus as a late one.

It may be asked: should not these, on account of their differences, be regarded as temporal species rather than races? May it not be said that in the course

of time one species of Micraster changed into another? To this question it is only possible to give the reply " We do not know, because we have no means of ascertaining, first whether cor-bovis, if crossed with coranguineus would have yielded fertile offspring, or, had the conditions that led to the formation of coranguineus been reversed, whether the latter would have gradually reverted to cor-bovis. But we can say that, so far, no breeder, scientific or otherwise, has succeeded in producing an animal variety which is not perfectly fertile when crossed with the parent form, although some of these varieties differ greatly in appearance from the parent."

Thus the results of genetical work lead us to doubt whether cor-bovis and cor-anguineus, despite their differences in form, belong to different breeding species.

We know that all species experimented on are capable of assuming considerable variety of form. Some breeds of pigeon differ from others in appearance more than some wild species differ from other wild species. We do not know what has caused the blue-rock pigeon to vary or mutate so as to give rise to all the domestic breeds, but we do know that the form of every organism, although determined mainly by its hereditary outfit, is affected by external influences. As Robson and Richards put it (" The Variations of Animals in Nature," (1936) p. 19) : " That animals are more or less ' plastic ' or modifiable by the environment in their structure, reactions, and physiological properties and activities is a fact of general knowledge."

An instance of this plasticity of the organism came to my notice in India. Ordinarily the stem of the rice plant attains a length of three or four feet, but when

planted in a depression which becomes a small lake during the rainy season, each rice plant, in order to keep its head above the slowly rising surface of the water, grows a stem which may attain a length of twenty feet or more. Seed from such heads when sown in an ordinary paddy field produces plants having a stem of normal length.

Schmankewitsch discovered that the three crustaceans named Artemia milhausenii, Artemia salina and Branchipus stagnalis are all forms of one and the same species, which takes the first form in very salt water, the second in moderately salt water, and the last in fresh water.

Change of food may greatly modify the form of an animal. Kukuchi discovered (Jour. Fac. Sci. Imp. Univ. Tokyo, 1931) that the rotifer, Brachionus pala, when fed with the alga Scenedesmus, grows spines, and that this effect is reversed if the spiny form be fed on Polytoma.

There is abundant evidence that unfavourable environmental conditions may exercise considerable effect on the form of an organism. Semper found ("Animal Life," p. 161) that the shell of the common pond-snail, Limnaea stagnalis, if raised in crowded conditions does not attain its normal dimensions: to attain the proper size (18 inches) each individual requires 2000 cubic centimetres of water; the shell of individuals allowed only 100 cc. attained a length of only 6. inches.

The familiar gold fish is derived from the greyish crucian carp (Carassius auratus). A number of varieties of this have been bred. According to Tornier these breeds are the result of insanitary conditions under which gold fish are reared in China. The following

SOME VARIETIES OF GOLDFISH.

THE LIONHEAD. Note the large head, due to wart-like excrescence, and the lack of a dorsal fin.

THE COMET. Note the slim body, long tail and fins.

THE METEOR. Note the egg-shaped body, great dorsal fin, and lack of tail.

THE VEILTAIL. Note the shortened body, immense tail and long fins.

THE TELESCOPE. (Viewed from above after Gunther). Note the stalked eyes and the lack of the dorsal fin.
(The "telescope-eye" may appear in fantails, veiltails, Moors and other varieties).

More than a dozen distinct breeds of goldfish are known. Mr. T. C. Roughly writes ("The Cult of the Goldfish," p. 37): "The shape may vary greatly; some of the fins may be enormously developed or some entirely absent: the colour may range from pearly white to black; and the scales may be transparent, when the fish may exhibit a large range of beautiful colours."

All the varieties are less hardy than the normal form.

Meteor

Veil-tail

Lion Head

Comet

Telescope

passage is from Professor MacBride's very interesting article in "Evolution in the Light of Modern Knowledge": "Tornier discovered that the Chinese breeders rear their fish under the most insanitary conditions. In winter they are kept in small earthenware pots ranged by hundreds on shelves in dark and ill-ventilated huts, and in summer they are transferred to small dirty tanks overgrown with weed. In these tanks they spawn and much of the spawn dies, but amongst the portion that survives monstrosities of all degrees of intensity appear, and the most striking of these are selected as founders of the fancy breeds. In view of these facts Tornier has put forward the view that all the features in which these breeds depart from the normal type—elongated fins, doubled fins, absence of fins, protruding eyes, puffed skin, and short deep shape—are due to the same cause, viz. the weakening of the vital powers of the embryo by the abstraction of the necessary oxygen at an early and critical period of development."

Whether or not Tornier's theory as to the causes of the changes in goldfish be correct, there is no doubt that unfavourable conditions may exercise considerable effect on the form and organisation of individuals affected without changing these into new breeding species. All the varieties of goldfish are fertile *inter se* and with the parent type.

In view of these facts and the various forms assumed by the shell of the dog-whelk (Purpura), (see p. 157) it would be hazardous to assert that the earlier and later forms composing the fossil series of Micraster and other invertebrates cited by Dr. Davies are not, in each case, members of one and the same physiological species.

How then are we to deal, as regards nomenclature, with extinct types that have undergone changes like those exhibited by Micraster, Zaphrentis, etc.?

The present Linnaean system is designed for organisms in existence contemporaneously. It seems advisable that, when dealing with species in time, we should give a non-committal name to each lineage of which there is unequivocal evidence, thus the Micraster lineage under discussion might be given the names of its earliest and latest terms : M. cor-bovis-cor-anguineus Probably the number of such undoubted lineages would not be very great. We should have to distinguish between them and such hypothetical lineages as are afforded by the pedigrees drawn up by Dr. Davies for Equus and Halicore. In the case of Micraster the fossils present a continuous series from cor-bovis to cor-anguineus, each term of which merges into its successor so gradually that in some specimens it is not easy to determine the term to which they should be assigned. Very different is the case of Equus. (see p. 143).

The most that can be said of the so-called pedigree of the horse is that possibly each member of the series composing it was derived from the preceding member.

Even if it were proved that Micraster cor-bovis would not have produced fertile offspring if crossed with cor-anguineus, i.e. that they are good physiological species, it would not be legitimate to infer therefrom that fish have in the past been changed by degrees into elephants. There would be no more justification for this than to infer that a man can run 100,000,000 yards in 11,000,000 seconds because he has been timed to run 100 yards in 11 seconds. Nevertheless this is what evolutionists do. (See p. 73). Dr. Davies does more. He writes

(E. p. 124) : "The rate of change shown by these forms is far too slow to account for the evolution of the genus Micraster itself from one of the earliest Irregular Echinoids which lived a little before the middle of the Jurassic Period. Rapid evolution must have occurred during critical periods when conditions were rapidly changing." Because Micraster is seen to undergo modifications at a slow pace to infer that formerly it must have undergone them more rapidly is tantamount to assuming in the above illustration that the man ran the 100,000,000 yards at a much greater pace than he had run 100 yards! To assert that rapid evolution *must have* taken place is to beg the question. Indeed, it may be that the changes to which the Micraster fossils testify were unusually rapid because the environment in which they lived underwent rapid changes.

Dr. Davies' statement illustrates the effects of the adoption by biologists of the creed that there is practically no limit to the amount of transformation of which some organisms are capable. I submit that the correct scientific attitude is to make no assumptions, except purely as hypothesis, and to determine by experiment, observation and strict fossil proof what changes can be, or have been, effected.

The amount of actual change demonstrated by the known fossils is in every case inconsiderable and this is equally true of genetical experiments. So far as I am aware there is no PROOF that a new breeding species of animal has ever arisen in nature. (See chap. IV.).

Mr. H. R. Kindersley has been led by this fact to write a book entitled "Organic Evolution Provably False."

An Analysis of the cases of Evolution cited by Dr. Davies.

The cases of evolution adduced by Dr. Davies may be thus classified:

I. THE "EVOLVED" ARE STILL LIVING AND THE PARENT TYPES EXTINCT

Equidae	Halicoridae
Nassidae	Cypraeidae

In all the above cases the supposed evolution has not been proved. In none of them is it possible to name a fossil ancestor generally acknowledged to belong to another family.

II. THE "EVOLVED" AND THE PARENT ARE BOTH LIVING TO-DAY.

Placenta

The derivation of Placenta from Anomia is not proved.

III. THE "EVOLVED" ARE EXTINCT AND THE PARENTS STILL LIVING.

Valenciennesia	Certain Viviparids
Gryphaea	Certain Volutilithes

In these cases the supposed evolution is clearly degeneration preparatory to extinction.

IV. BOTH "EVOLVED" AND PARENT ARE EXTINCT.

Rudists	Inoceramus
Syringothyris	Zaphrentis
Micraster.	

In the first three of these the supposed evolution was clearly degeneration, and this may well be true of the last two.

Dr. Davies has presumably collected the best fossil evidence of evolution obtainable.

Seeing that there are to-day in existence many thousand living species of animals, all of which are supposed to have evolved from ancestors very unlike themselves, is it not strange, if the theory of organic evolution be true, that in all of the cases, that is the nine in Classes III. and IV., in which the fossil evidence of modification is fairly strong, the "evolved" died out as soon as they were fully evolved? In the remaining five cases in which the "evolved" are still in existence the evidence of the supposed evolution is far from conclusive.

Dr. Davies has cited some excellent fossil evidence of the modification of species as the result of unfavourable environment, and has shown that the fossils, like breeding experiments, indicate that organisms are to some extent plastic, but I maintain that he has failed to furnish proof of the origin of a new family by a process of evolution.

Conclusion

Most of those who have read "Difficulties of the Evolution Theory," "Evolution and its Modern Critics," and the present volume will, I think, come to the conclusion that, although Dr. Davies has adduced many valuable facts, the difficulties not only remain but have assumed a greater magnitude in the light of the attempts made by that distinguished biologist and palaeontologist to meet them. In the present volume I have set forth further formidable difficulties. There are yet other difficulties—those of a psychic nature.

Dr. Davies writes (E. p. 248): "It must never be forgotten that however far has been carried the analysis of Life and Evolution in terms of Physics and Chemistry, the psychic side of life is untouched...That Consciousness, Memory and Personality have been gradually developed step by step with evolution of bodily structure seems unquestionable, yet we can frame no theory of how the two sides of this double process are connected, nor how their association first began."

Dr. Davies states that he has avoided discussion of the causes of evolution. He has, I think, acted wisely for two reasons.

First, there is no agreement among evolutionists as to these causes. "Without doubt" writes Professor M. Caullery (P. p. 7), to-day we feel farther from representing how evolution has been effected than forty years ago when the writer began to study Zoology."

Secondly, in the present state of knowledge to try to discover the causes of evolution is tantamount to trying to cook the uncaught hare!

Biologists are now chasing what may be a Will-o'-the-wisp. It is pathetic that two competent zoologists laboured for years in trying to show how vertebrates could be derived from an Arthropod. As recently as 1936 appeared a monumental work the aim of which is to ascertain what light the study of the cranial muscles of vertebrates throws on the genetic relations of the vertebrate phylum, and so on the phylogenetic history of man in the past. A reviewer of that book writes: "The skeleton, the central nervous system and many of the viscera have been studied from this point of view, but have yielded no information of great significance on this problem. When these systems have failed it is scarcely to be expected that the study of muscles would be more fruitful."

Not that these studies have yielded no return; the return would have been far greater had the studies been directed to less speculative matters. Surely the proper course is first to make sure of the amount of modification of which organisms are capable, and then to seek for the causes of such modification.

I see no harm in the theory of evolution in the fullest sense being adopted as an alternative hypothesis to that of creation, but I consider it suicidal to adopt evolution as a creed, to distort facts to cause them to conform to it, to brush aside facts not amenable to it, and to minimise difficulties.

Dr. W. R. Thompson well says ("Science and Common Sense" (1937) p. 223): "The fundamental difficulties about the theory of Evolution are, in fact,

not theological, but rational and experimental."

This distinguished zoologist also writes (p. 229): "The concept of organic evolution is very highly prized by biologists, for many of whom it is an object of genuinely religious devotion,* because they regard it as a supreme integrative principle. This is probably the reason why the severe methodological criticism employed in other departments of biology has not yet been brought to bear against evolutionary speculation. There are, however, indications that this criticism will not now be long delayed."

May this forecast be realised!

* Sir Peter Chalmers Mitchell, for example, writes ("News Chronicle," January 29th, 1938): "Evolution...has passed from a vague theory to an accepted fact." Sir Arthur Keith says ("Darwinism and its Critics," (1935) p. 53): "A belief in Evolution is a basal doctrine in the Rationalist liturgy."

GLOSSARY

ACETABULUM—Socket in hip-bone in which head of thigh-bone articulates.

ACROMION—The outer extremity of the shoulder blade.

AMNIOTE—An animal having an amnion, i.e. a reptile, bird, or mammal.

APONEUROSIS—The membrane forming the sheath of a muscle, or connecting a muscle with a tendon.

ARCHAEOSAURIAN—A primitive type of reptile.

ARTICULAR—The bone of the lower jaw of reptiles that articulates with the skull.

ARTIODACTYL—A hoofed animal having an even number of toes on each foot.

ATROPHY—A wasting away.

BUCCAL—Appertaining to the mouth.

CHROMOSOMES—Paired structures into which the cell nucleus breaks up when dividing. The number varies with the species.

COLUMELLA AURIS—A rod-like bone in the ear of amphibia, reptiles and birds.

DENTARY—The bone of the lower jaw that carries the teeth.

DIAPHRAGM—Midriff or membrane dividing the thoracic from the abdominal cavity.

ENDOCHONDRIAL—Within cartilage.

ENDOSTYLE—A ciliated groove on the floor of the pharynx of ascidians and amphioxus.

EPICRANIAL—Appertaining to the scalp.

EPIGLOTTIS—The erect cartilage at the base of the tongue which, during the act of swallowing, closes down over the opening of the windpipe.

EPIPHYSES—Extremities of long bones that ossify separately.

FEMUR—Thigh-bone.

FORAMEN—(Pl. Foramina) An aperture.

GENE—The unit of heredity. The genes are hypothetical parts of the chromosome which are believed to affect development.

GENETICIST—A scientific breeder.

GERM PLASM—The part of the organism which gives rise to new individuals.

HUMERUS—The bone of the upper arm or the upper fore-leg.

HOLOCENE—A term applied to the Quarternary or Pleistocene Period.

ILIUM—The upper bone of the pelvis (q.v.)

ISCHIUM—The lower bone of the pelvis (q.v.)

LANUGO.—The foetal hair of mammals.

MARSUPIAL—A mammal that typically has a pouch in the abdomen in which the young are carried. e.g. kangaroo.

MESOZOIC—The second of the great geological periods in which living organisms have existed.

METABOLISM—The chemical processes that take place in the organism.

METAZOA—All the animals composed of more than one cell.

MONOTREME—A mammal that lays eggs.

NOTOCHORD—The axial rod in the dorsal region of the embryo derived from the middle germinal layer, forming the primitive basis of the spine.

OBTURATOR FORAMEN—A hole closed by a membrane.

OCCIPITAL CONDYLE—The bony process by which the skull articulates on the backbone. Birds and living reptiles have one condyle, mammals have two.

ONTOGENY—The history of the development of an individual.

ORGAN OF CORTI—A complicated vibrating organ in the mammalian ear.

PALAEOZOIC—Sometimes called the Primary, the first of the great geological periods during which animals and plants are known by their fossils to have lived on the earth.

PELVIS—Hipbone. This is made up of an os innominatum on each side. Each os innominatum is composed of three bones—an upper, the ilium, a lower, the ischium, and the middle, the pubis, which joins with its fellow of the other os innominatum.

PERISSODACTYL—A hoofed mammal having an odd number of toes on each foot.

PLACENTA—The organ, peculiar to placental mammals, whereby the foetus derives nourishment from the mother.

PLACENTAL—A mammal having a placenta, as opposed to marsupials and monotremes.

PHYLOGENY—The term applied by evolutionists to the supposed ancestral history of an animal.

POLYPLOIDY—The condition in which the normal number of chromosomes (q.v.) is exceeded.

PREMOLARS—The cheek teeth of the adults that replace the milk teeth of the young mammal. The front or cutting teeth are called incisors, the pointed teeth next to them are the canines, then come the premolars, and finally the molars.

PRIMARY PERIOD—See Palaeozoic.

PRIMORDIUM—The first distinguishable stage in the development of an organ.

PROTOZOA—The animals whose bodies are composed of a single cell, as opposed to all the others—the Metazoa.

PUBIS—See Pelvis.

RADULA—The rasping tongue of molluscs.

SACRAL—Appertaining to the Sacrum, which in man is a bone composed of five vertebrae. In man the six neck vertebrae form the cervical vertebrae, then come the dorsal vertebrae, followed by the lumbar vertebrae, after them comes the sacrum, followed by the coccyx composed of four fused vertebrae that end the spine.

SCAPULA—Shoulder-blade.

SECONDARY PERIOD—See Mesozoic.

SPORANGIA—Organs containing the spores (reproductive organs) of plants.

STERNUM—Breast bone.

STIRPS—A name sometimes given to a group of animals larger than a family and smaller than an order. The groups into which animals are divided by systematists are, in descending order of magnitude: phyla, classes, orders, families, genera and species.

SYMPHYSIS—The area in which two bones are joined to one another.

SYSTEMIC CIRCULATION—The general blood system of the body as opposed to the respiratory system.

TAXONOMIC—Classificatory.

TERTIARY—The third of the great geological periods in which organisms are shown by fossils to have inhabited the earth.

TIBIA—The shin bone.

TRANSFORMISM—The doctrine that all organisms are derived from a common ancestor, or two or three ancestors.

UMBO—The point on a bivalve shell above the hinge.

UNGULATA—The Order of hoofed mammals, including both Artiodactyla and Perissodactyla (q.v.)

XYLEM—The vessels and fibres forming the hardest part of a tree trunk.

ZYGOMATIC ARCH—An arch of bone on each side of the skull in mammals.

INDEX

Abel, Prof. O., 149, 153.
Abbeville jaw, 93.
Aceratherium, 142.
Acila, 139, 171.
Adaptive radiation, 66.
Agazziz, 123.
Agria, 167.
Algae, supposed pre-Cambrian, 109, 114.
Alectryonia, 185.
Ambiens muscle, 61, 80.
Ammodiscus, 176.
Anchitherium, 147.
Angiospermophyton, 135
Annandale, 179-183.
Anomia, 158-160.
Anomiidae, 158-160, 202.
Ant-eater, American, 57.
Aplysia, 63.
Aponeurosis, epicranial, 61.
Araucarites, 133.
Archaeocyathus, 118.
Archaeopteryx, 75-77.
Archaeornis, 76.
Archaeozylon, 110.
Arches, visceral, 66.
Arius, 97.
Artemia, 198.
Ascidia, 42.
Atavism, 69.
Atikokonia, 109.
Australopithecus, 93.

Bailey, Dr. L. W., 133, 173.
Baldwin, Dr., 20, 21.
Barrande, 123.
Barrett, 87, 110, 120.
Bateson, 179.
Bather, Dr., 191.
Beebe, Mr. A., 24.
Belogolovy, 16.
Beltina, 111-114.
Berg, Prof. L., 16, 69, 84, 95, 156, 176.
Bergson, 18.
Birds, derivation of, 74-80.
——, mortality among, 78.
Birmingham, 57.
Blind cave dwellers, 58.
Blind spot, 37.
Blood circulation, embryonic, 28 *et. seq.*
Brachionus pala, 198.
Brachiopods, fossil, 174.
Bradley, Mr., 119.
Branchipus, 198.
Brogniart, 172.
Broili, 175, 176.
Brooks, Prof. W. K., 117.
Broom, Prof., 52, 53, 86.
Bullia, 63, 168.
Bythinia, 179.

Calaveras skull, 93.
Cambrian fauna, 107.
Camels, 147.
Carassius auratus, 198.
Cardium edule, 179.
Carelozoon, 111.
Carolia, 158, 159.
Carp, crucian, 198.
Carruthers, Mr. R. G., 192, 194.
Castenedolo skull, 93.
Caullery, Prof. M., 10, 204.
Cave dwellers, blind, 58.
Cayeux, 110, 111.
Cervulus, 55.
Cestodes, 48.
Cetacea, 151, 152.
Chamberlin, 117.
Chamidae, 166.
Change of function, 65.
———— — habit, 67.
Chapman, Mr. F., 176.
Chelonia, 151.
Chimpanzee, 92.
Cirripedes, fossil, 123.
Clichy skeleton, 93.
Climate, past changes, 71.
Coccyx, 33, 35.
Cochlostyla, 158.
Cockle, 179.
Collar bone, 55.
Collenia, 109.
Convergence, 66.
Cooke, Dr., 154.
Correlation, principle of, 65.
Corti, organ of, 85.
Corycium enigmatum, 110.
Cox, Mr. R. C., 165-7.
Crampton, 72.
Creationism, 1.
Creation, units of, 8, 9.
Crew, Prof., 27.
Crisp, Dr., 77.
Cuénot, Prof., 52, 53, 125.
Curie-Joliot, 102.
Cuvier, 65.
Cypraeacea, 160-3, 202.
Cypris, 41.

Dacqué, 65.
Dadoxylon, 133, 134.
Dall, 179.
Daly, Prof., 116.
Dana, 175.

D'Archiac, 123.
Darrah, Dr. W. C., 133.
Darwin, 56, 61, 92. 94. 108. 121. 123, 124.
David, 112, 114.
Davidson, 174.
Davies, Dr. A. Morley, 1, 2, 8-10, 17-20, 22, 26-41, 51, 52, 61-82, 85-94, 106, 122-7, 137-45, 149-53, 158-204.
Davies, Col. L. Merson, 124, 177,
Dawson, 108.
de Beer, Dr., 67.
Deer, lateral toes of, 55.
——, red, 156.
Denudation, rate of, 131.
Dépéret, 123, 124, 142, 145.
D'Halloy, 122.
Diaphragm, 85.
Dicynodon, 84.
Didelphys, 68.
Diphyllobotrium, 48.
Distomum, 47.
Dog-whelk, 167, 191.
———— family, 167-169.
Dorsanum, 168.
Drosophila, 59, 60.
Dubois, 92.
Dugong, 149-53.

Ear muscles, 61.
Earland, Mr. A., 176.
Earthworm, 28.
Echidna, 57.
Echinoderms, 118.
Edentata, 57.
Embryonic remains, 60, 62.
Endoparasites. 47-50.
Endostyle, 66.
Enhydris, 25.
Eohippus, 139-148.
Eosiren, 150.
Eotheriufm, 149, 150, 152, 156.
Eotheroides, 149.
Eozoon, 108.
Epiglottis, 66.
Epihippus, 140, 142.
Equidae, 139-48, 202.
Equus, 139-48, 200.
Eurypterid, fossil, 111, 114.
Evolution, clandestine, 67.
———, convergent, 67.
———, disorderly, 67.
———, parallel, 67.
———, zigzag, 67.
Evolutionism, 1.
Exogyra, 184-6.
Extrapolation, extravagant, 73.
Eye of flatfish, 44.

——, pineal, 37.
——, vertebrate, 36-40.
Fabre, H., 50, 100.
Families, origin of, 137-169.
Felichthys, 97.
Fermi, Prof., 102.
Fishes, cichlid, 97.
Flatfishes, 43.
Fleischmann, Prof. A., 24, 125.
Fleming, Sir A., 106.
Foraminifera, alleged fossil, 111.
Forbes, E., 123.
Fossils, absence of Pre-Cambrian 107-121.
Foxhall jaw, 93.
Frog, edible, 11.
Furbringer, 76, 84.

Gager, 5.
Gammarus, 59.
Garrett, 73.
Garstang, Prof. W., 42.
Gastrornis, 77.
Geological deposits, dating, 101-5.
Gene complex, 59.
Gillespie, A. S., 6, 20.
Girvanella, 176.
Goldenberg, 172.
Goldfish, 185, 192.
Goldschmidt, G., 14.
Gomborrow, Mr. J., 73.
Goodrich, Prof. E. R., 11, 63.
Gorilla, 90.
Gottshick, 156.
Grabau, 118.
Gregory, Prof. J. W., 87, 110, 111 120.
———, ——, W. K., 87, 88, 142.
Gruner, 109.
Gryphaea, 184-6, 202.

Haack, 95.
Habit, change of, 67.
Haeckel, 94, 158.
Hagen, 172.
Haldane, Prof. J. B. S., 16, 23, 24, 129, 130.
Halibut, 44.
Halicoridae, 149-53, 200, 202.
Halitherium, 150.
Handlirsch, 173.
Hare, sea-, 63.
Hayes, 54.
Hazay, 154.
Hebbert, Mr. G. K., 172.
Heilmann, 76.
Heilprin, 172-4.
Helateles, 140, 141.
Helisonia, 157.

Hendeigh, 78.
Hendricks, Miss, 134.
Heron-Allen, Mr., 176.
Hesperornis, 75, 106.
Hilgendorf, 155-6.
Hipparion, 144, 147.
Holmes, Prof. A., 103, 105.
Holtedahl, 109.
Homogalax, 139-41.
Hopkins, Sir G., 6.
Horse family, 130-48.
——, toes of, 54, 69.
House martin, 100.
Howchin, 114.
Hurst, Dr. C. C., 5, 14, 15.
Hyatt, 155, 174.
Hybrids, 15.
Hyolithellus, 175.
Hyolithes, 175.
Hyrachyus, 139-42.
Hyracotherium, 141, 147.

Ibalia, 49.
Ichthyornis, 75.
Ichthyosaurus, 70, 151.
Ingersoll, 157.
Inoceramus, 186-8, 202.
Instincts, difficulties, 97-100.
Intersexes, 13.
Irreversibility, 68-70.

Jelly-fish fossils, 117.
John Dory, 44.
Joliot, Mdme., 102.
Joly, Dr., 102-3.

Kanam jaw, 93.
Kanjera jaw, 93.
Keith, Sir A., 34, 67, 93, 206.
Kindersley, Mr. H. R., 201.
Kleinschmidt, 95.
Koenigswald, Dr. G. H. R., 93.
Kraus, 149, 153.
Kraussel, Dr., 110.
Kukuchi, 198.
Kunitoma, 35.

Lane, Dr., 116.
Lang, Dr. W. D., 185.
Lanugo, 91.
Lead isotopes, 102.
Leakey, Dr., 143.
Lemoine, Prof. P., 177.
Liesegang, 109.
Life, origin of, 4-9.
Limnaea peregra, 154.
——— truncatula, 47.
Limnaeidae, 163-5.
Limnoclanus, 69.
Lingula, 70.
Linnaeus, 94.
Litopterna, 146.
Liver fluke, 47.
Lophiodon, 139-41.
Lophiodontidae, 140.
Lull, 142.
Lunn, Mr. Arnold, 23, 25, 129.
Lymantria, 13.

MacBride, Prof. E. A., 11-15, 196, 199.
Macrauchenia, 146.
Mammal-like reptiles, 81-84.
Mammals, origin of, 81-88.
Man, 89-96.
——, Heidelberg, 65.
——, Piltdown, 65.
Manatee, 149-53.
Marrat, F. P., 154.
Marsupials, 87.
McAtee, G. L., 78.
McCabe, Mr. J., 120.
McIndoo, 58.
Matthew, Dr. G. F., 133.
———, Prof. W. D., 113, 125, 140, 144, 145.
Megalosaurus, 87.
Melanopsis, 179.
Merychippus, 144.
Mesohippus, 144.
Metamorphosis, 39-48.
———, of Asclidian, 42.
———, — butterfly, 45.
———, — frog, 43.
Metaxytherium, 150.
Metzger, Dr., 109.
Miall, 68.
Micraster, 191, 195-201, 202.
Mitchell, Sir P. C., 206.
Moeritherium, 149-53.
Monotremes, 57, 86.
Morgan, T. H., 60.
Mosasauria, 152.
Moth, gipsy, 13.
Murchison, 123.
Murphy, Dr. John, 90.
Mysis, 69.
Myxocephalus, 69.

Nascent organs, 51-58.
Nassa, 154, 168.
Nassidae, 167-169, 202.
Nauplius larva, 40.
Nautilus, 174.
Neaverson, Prof., 184.
Nematocysts, 63.
Neritina, 179.

Neumayr, 179.
Newlandia, 109.
Noe, Dr. A. C., 135.
Nopsca, 76.
Nothotherium, 88.
Nucula, 139, 170.
Nuculidae, 170.
Nudibranchiata, 62.

Odontopteryx, 75.
Oldoway man, 93.
Olenellus, 118.
Olmo skull, 93.
Opossum, 68.
Orang, 90.
Oriole, 98.
Oriolus, 98.
Orohippus, 143, 144.
Orthoceras, 110.
Orthotomus, 97.
Osborn, 142.

Pachynolophus, 147.
Palaemonetes, 181.
Palaeontological record, 122-36.
Palaeotherium, 141.
Palaplotherium, 141.
Palephemera, 173.
Paley, 72.
Panniculus carnosus, 61.
Paralhippus, 144.
Parallel development, 66.
Parasitism, 39-50.
Payne, 59.
Plesiosauria, 151.
Pictet, 123.
Pig, toes of, 55.
Pineal eye, 37.
Pithecanthropus, 65, 90, 92.
Placenta, 159-60, 202.
Planorbis, 69, 155-6.
Platypus, 57.
Plesippus, 144-5.
Pliohippus, 144.
Ploceus, 99.
Polytoma, 198.
Pre-Cambrian fossils, alleged, 108-16.
Price, Prof. McCready, 171.
Primates, 67.
Principles, biological, 65-73.
Proteus, 59.
Protogene, 5.
Protohippus, 144.
Pterodactyls, 76-84.
Pterophyllum, 44.
Pteropods, 175.
Pterosaurs, 76-84.
Purpura, 154, 191, 199.

Pyrgoma, 176.
Pycraft, W. P., 77.
Pythonomorpha, 152.

Quenstedt, 156.

Racovitza, 58.
Radiolaria, 110.
Radium, 102.
Rana, 11.
Rastall, Dr. A. H., 120.
Raymond, Dr. P., 107-14.
Redlichia, 118.
Recapitulation hypothesis, 26-38.
Reptiles and Birds, 74-80.
Retina, innervation of, 36-8.
Rhinocerotidae, 140.
Rhinoderma, 97.
Rhumbler, L., 176.
Rice plant, 197.
Richards, 197.
Robson, 176 197.
Romer, Prof., 140-1.
Roth, Julius, 105.
Rowe, Dr., 195.
Rudists, 165-7, 202.
Rust, 111.

Sacculina, 41-4.
Sandberger, 156.
Salts in sea, 104.
Saw-fish, 70.
Scenedesmus, 198.
Schmankewitsch, 198.
Schilder, Dr., 160-2.
Schistosoma, 49.
Scudder, 172-3.
Sea-cows, 149-53.
Sea-squirts, 42.
Sedgwick, 123.
———, Adam, 109.
Sederholm, 110.
Seeley, 77, 83.
Selaginella, 17.
Semi-lunar fold, 60.
Semper, 157, 198.
Sergi, 95.
Seward, Sir A., 108, 110, 113, 134-5.
Sexton, 59.
Shann, 6, 20.
Sharks, 70.
Sidneya, 112.
Sinanthropus, 93.
Sirenia, 148.
Sirex, 49.
Skates, 70.
Smith, Dr. B., 188-9.

Sodium in sea, 104.
Sollas, Prof., 104.
Sphenodon, 37.
Spiny dog-fish, 70.
Spirillina, 176.
Spisula, 126.
Splint bones of horse, 54.
Stamp, Dr., 141.
Styliola, 175.
Syringothrys, 189-91, 202.
Systemodon, 139-41.

Taeniopteris, 87.
Taia, 180-4.
Tail, human, 33-6, 60.
Tailor-bird, 97.
Tape-worm, 48.
Tapiridae, 140.
Teeth in foetal whales, 56.
Testacella, 62.
Tetraclaenodon, 140.
Therapsid reptiles, 81.
Thoatherium, 146.
Thomas, 190.
Thompson, Dr. W. R., 131, 205.
Thomson, Sir A., 25.
Thorium, 101-4.
Thyroid cartilage, 66.
——— gland, 66.
Tilley, Dr. C. E., 120.
Tillyard, 112-4.
Titanophasma, 173.
Toes, 54-6.
Tornier, 198-9.
Transformations of animals, 17-25.
Tree-kangaroo, 70.
Trematoda, 48.
Trichoniscus, 58.
Trilobites, 116-8.
Tritylodon, 82.
Troglohyphantes, 116-8.
Tulotoma, 179.
Tunicata, 42.

Unio, 179.
Units of creation, 8-10.
Uranium, 101-4.

Valenciennesia, 163-4, 202.
Variation, non-adaptive, 72.
Velutinopsis, 163-4.
Vermiform appendix, 57.
Versluys, 76.
Vestigial organs, 51-4.
Vialleton, 17, 22, 56, 61, 72, 137 138, 142, 152, 153.
Vitrina, 158.
Viviparids, 178-83, 202.
Volutocorbis, 188.
Volutilithes, 188, 202.
Volutospina, 188.

Wallace, A. R., 59, 90, 158.
Walcott, 108-17, 175.
Watson, Prof. D. M. S., 191-2.
———, E. L. Grant, 45, 63.
Weller, Stuart, 112.
Wells, Mr. H. G., 125.
Westenhofer, Prof. M., 94.
Whales, toothless, 56.
Whitlock, Dr. H. P., 113.
Whitney, Mr. D. J., 103
Williamson, W. C., 122.
Wilmore, Mr. A., 192.
Winge, 59.
Woods, Prof. H., 184, 186-7.
Woodward, 123.
Woodward, Sir A. S., 70, 76.
Wood-wasp, 49.
Wynwardia, 87.

Zaphrentis, 191-5, 202.
Zeuglodon, 151.
Zittelia, 161.

REFERENCES TO BOOKS FREQUENTLY QUOTED

B. Encyclopaedia Britannica. 14th Edn. 1929.

D. Dewar, D. Difficulties of the Evolution Theory, 1931.

E. Davies, A. Morley, Evolution and its Modern Critics. 1937

F. Dewar, D. "A Critical Examination of the supposed fossil Links between Man and the Lower Animals," Journal of the Transactions of the Victoria Institute. vol. LXVII. (1935) p. 157.

M. Dewar, D. Man: A Special Creation.

N. Berg, L. S. Nomogenesis, or Evolution determined by Law. Eng. trans. by J. N. Rostovtsow. (1936).

O. Vialleton, L. L'Origine des Etres Vivants. 15th Edn. (1930) Paris. Librarie Plon. 8. Rue Garancière. 6e

P. Caullery, M. Le Problème de L'Evolution, 1931. Paris.

S. Seward, A. C. Plant Life through the Ages. 2nd Edn. 1933.

V. Vialleton, L. Membres et Ceintures des Vertébrés tetrapodes. 1924. Paris. Librarie Octave Doin, 8, Place de L'Odeon, 8.

Z. Zittel, K. A. von. Text Book of Palaeontology, trans. by C. R. Eastman, 2nd. Edn. 3 vols. 1915-1924.

OTHER BOOKS ON EVOLUTION

by

Douglas Dewar

DIFFICULTIES OF THE EVOLUTION THEORY
192, viii pp. 7 illustrations. 12/6.

"The author makes some shrewd criticisms." *Geological Magazine.*

"Mr. Dewar's arguments are always interesting even if they are not wholly convincing." *Lancet.*

"Vibrant with scientific facts based on careful investigations . . ." *Christian.*

"A fascinating thought-provoking book...full of sound common sense." *Civil and Military Gazette.*

MAN: A SPECIAL CREATION. 122 pp. 3/6

"Dewar . . . is a distinguished naturalist of long and varied field experience...we are of opinion that his observations and ideas cannot be neglected." *Quarterly Review of Biology.*

"In concrete instance after instance, the author shows how the physical facts of man's nature do not square with the evolution theory of his origin." *Defender.*

"As one would expect from Mr. Dewar's scientific training he deals very fully with the problems on biological lines. He never overstates his case, or puts forward as facts things that are only theories. It is just the book to put in the hands of the young people of to-day." *Church Newspaper.*

A CHALLENGE TO EVOLUTIONISTS. 63 pp. 2/6

"Interesting and illuminating." *Record.*

"Let us see what reply the pro-Evolutionaries give to this destructive challenge." Lt.-Col. P. W. O'Gorman, M.D. on 7th. August, 1937, in *Glasgow Observer.*

"A formidable array of just those problems and questions which no dogmatic evolutionist has ever yet attempted squarely to face or fairly to answer." Lt.-Col. L. Merson Davies, F.R.S.E., F.G.S., in *Evangelical Quarterly.*

OBTAINABLE FROM THE PUBLISHERS.